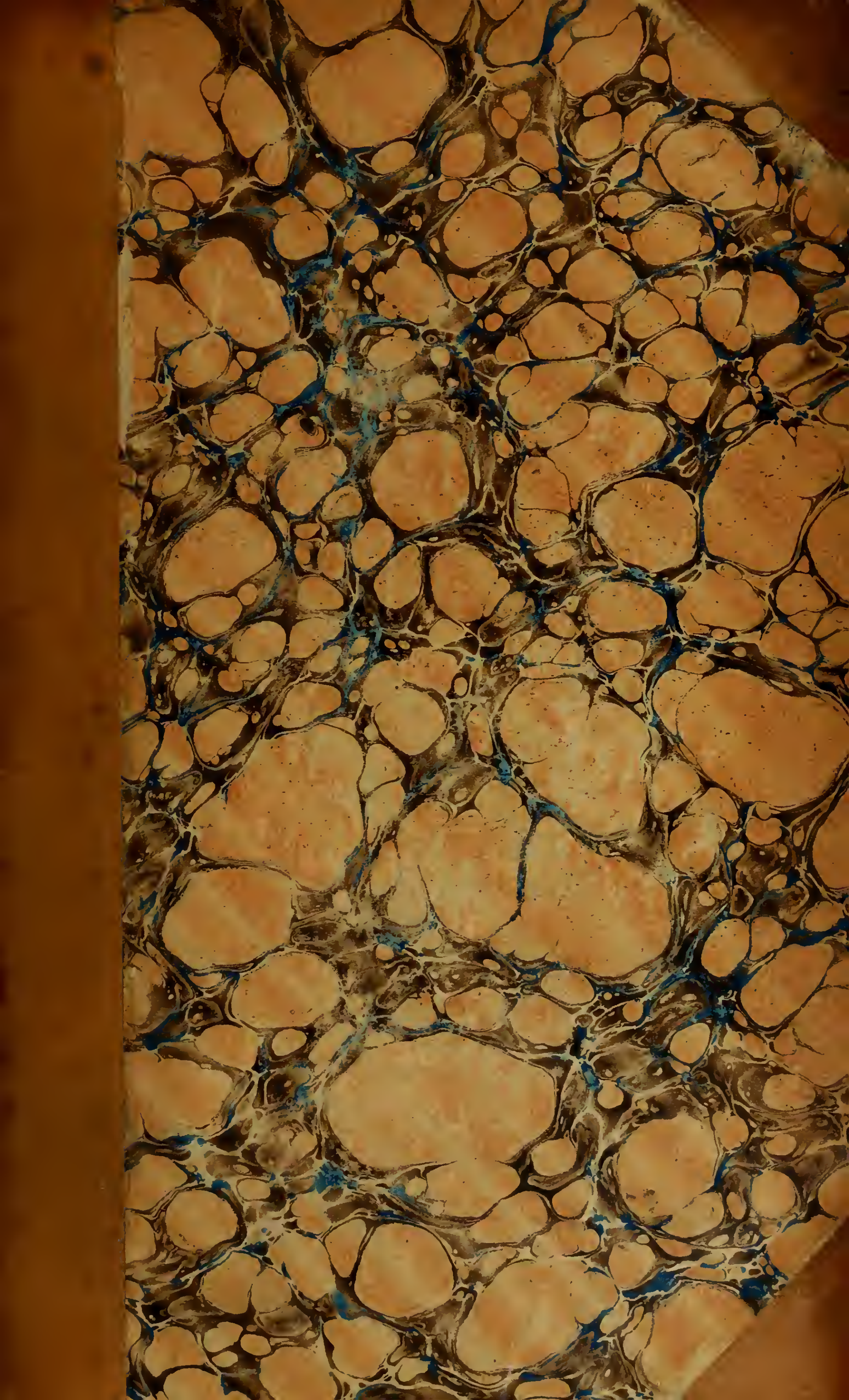

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OF THE
FRANKLIN INSTITUTE
OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

DEVOTED TO

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Civil Engineering, the Arts and Manufactures.

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JANUARY, 1864.

CIVIL ENGINEERING.

For the Journal of the Franklin Institute.

Papers on Hydraulic Engineering. By SAMUEL McELROY, C. E.

(Continued from vol. xlvi, page 369.)

No. 4.—CITY SEWERAGE. (Continued.)

THE general characteristics of sewage disposal, and of ancient and modern practice, having been considered, it remains to deduce from this subject prominent principles of arrangement and construction in reference to general and specific plans, for the main trunk or *arterial* system, and the lateral or *venous* system, and their appurtenances, which will be noticed in this order.

Topography.—In any general plan of city sewerage, which properly includes the disposal of rainfall and the minor supplies, a careful study of the entire local topography is requisite, not only as to the area of the city proper, but as to all the lines and slopes of natural drainage which connect with it.

This forms the proper basis of the plans of main trunks or arteries, as to their location along the flow lines of the ravines, and prominently as to their sizes and forms, since it is shown that any adequate provision for storm flow, must be largely in excess of the house sewage discharge.

It also follows that a study of the primary, secondary, and tertiary ravines and their water slopes, controls the subdivision of these arteries in length and size, with reference to the special areas drained.

In this sense, the arterial system of any correct plan of sewerage, is independent of populated area, or population, and if properly arranged at the outset will never give occasion for re-arrangement; and the propriety of careful and substantial construction, so far as the system may be carried out at any intervals of time towards its final completion, is clear.

The importance of this principle might be strongly urged; it is a safeguard against all future trouble; the accidents of populative increase, or change of water supply, or improvements in pavement, always find whatever is completed adequate to all demands; and the long schedules of public and private damages, which are thus avoided, are negative witnesses of its superior economy, as a question of cost.

Outlets.—The places and method of discharge for these arteries, vary in convenience and propriety in different localities, and demand careful scrutiny. The question of atmospheric and hydraulic contamination, has justly received in cities like Amsterdam, Hamburgh, Paris, London, Boston, and many others, exposed to tidal re-actions or sluggish receptacles, the most earnest attention, and in some cases induced very costly constructions of artificial intercepting outlets.

In the case, however, of tidal streams of considerable size, or any body of water in constant motion, there is room for question, in more than one instance, whether the cost of interception would be justified, with a system of sewage disposal properly charged with water supply, since contact with large bodies of cool water and aeration, have a powerful protective influence over the objectionable qualities of sewage. There may be an extreme in apprehension as in neglect of proper care, and popular prejudices are often permitted to overrule intelligent and safe counsels, as we have had several occasions to notice, where valuable and economical water supplies were rejected from erroneous doubts of their quality, on assumed impregnation. Both at London and Paris, it is possible that a slight contraction of the Thames and Seine, so as to ensure a positive current, would be safer in a sanitary view, than the extended intercepting areas of sewage flow, which, in the former case particularly, is almost certain to be returned to the city with each flood tide, and deposited at slack water.

Sewer discharge into sluggish basins or receptacles of any kind, which accumulate deposits, is highly objectionable.

The use of pumping power to obtain an artificial outlet, where natural fall is denied, is a very simple, effectual, and often economical process. There is no mechanical difficulty in pumping sewage, and with the high standard of duty in engines of correct design, the annual cost is not formidable, and may be largely overbalanced by resort to other expedients for drainage, under an erroneous impression of cost. If, at Chicago, no advantages in a mercantile way, and in other respects, were realized by the gradual elevation of the streets and buildings, the use of pumps to obtain outfall would have been clearly justified, as a matter not only of economy but of superior efficiency. At New Orleans, unquestionably, a system of sewers with artificial outfall, would redeem the sanitary reputation of that important city.

Advantage is sometimes taken, with success, of intermittent water supply, as at Hamburgh, Jersey City, Savannah, and other places, to flush the main sewers, where natural inclination cannot be had, and in the case of Savannah particularly, the results, with a very slight inclination, prove satisfactory. In the case of arterial systems exposed to tidal re-actions, the use of simple outfall gates may operate as flushes, with certainty. But it is evident, that intermittent or automatic arrangements of this kind are not comparable with constant, self-acting grades, and should be restricted to cases of special necessity.

Location and Form.—The proper location of the sewage arteries is along the drainage ravines or flow lines of the principal water shed slopes of the general area to be provided for; and they should be adapted in form, size, and grade, the three elements of discharge, to the ultimate requirements of their special supply areas.

The form of trunk mains, is somewhat controlled by the materials used in their construction, circles and ovals being most convenient for brick-work, where a different section is required for stone-work of moderate cost.

For sewers which exceed five feet in diameter, stone-work lined with brickwork, with a compound section, nearly vertical at the sides, combines strength. durability, and facility of flow. For those between five feet and forty-two inches, (which is the proper limit to arterial size,) brick-work is the most convenient, and the oval form with the minor curve at the base, the best; but the selection of sewer and aqueduct bricks should be made with great care, to secure thorough burning, since when exposed to dampness they need very compact section, nearly impervious and indestructible.

For the more common size of trunk sewers, the oval form has the advantage of a constant and concentrated current during the ordinary periods of house discharge, in which the city water supply is carried off; and of an increasing discharge section for storm flow, as the water area is increased; it also admits examination and repair more fully than a circle of the same area.

By reference to the Table given under the head of *Rainfall*, p. 454, vol. lxxvi, the relations between section, inclination, and discharging capacity, will be found as guides for the arrangement of the primary, secondary, and tertiary mains in subjection to their drainage areas; and the gradual concentration of drainage, towards the principal artery, from its branches, will control its conditions of arrangement.

Tubular System.—The arterial system which has been presented, belongs to the more general and rude division of the work, while the precepts of theory and practice point out the venous or tubular system, as the most delicate in its order, and the most vital in its effects.

For all cities of any important size, the use of trunk mains is desirable and proper, in their legitimate places, and should not be neglected; but the laws of sewage flow, also demand, in all the subordinate branches of discharge, the use of laterals, which will properly solve the problem of "adequate disposal in the shortest time," and these should be pipes.

The same principle of arrangement which establishes the conditions of a trunk main by its drainage area, should also control the several groups of pipe feeders, which convey to it their several district accumulations, by their own laterals and mains, and which vary from 6 to 24 inches in diameter, and range from 1 to 115 acres in drainage capacity, on inclinations of 1 in 60.

This network of veins, not only admits the most careful adjustment, and secures the greatest economy in first cost, but also illustrates the principle of concentration of flow, by which the ordinary water supply is enabled to act with full power on all the substances which disposal of sewage includes.

In the arterial system, moderate inclinations are, as a general rule, controlled by natural formation along their lines, and involve a condition of safety, on this account, under the discharge of large bodies of water; but in the venous system, greater inclinations are natural and advantageous to the reduction of calibre.

But since it has happened in some cases, where trunk mains have occupied the place of pipes, that their outlets have been surcharged under storm flow, it is erroneously concluded that their size was inadequate, whereas the fault lay in their great rapidity of lateral supply under objectionable grades, which accumulated on their outlets, suddenly and uselessly, masses of water, that might have been properly disposed of by a more correct system. Pipes, therefore, are useful to their trunk mains, during storm flow, in preventing effects which have been used as arguments against their adoption; and they do this, without risk of being themselves surcharged.

In every arrangement of city distributing water pipes, the lengths of large mains form a limited proportion of the aggregate length of pipes laid. Of 120 miles of pipes specified in the Brooklyn Water Works contract, 64 miles were 6 inches in diameter. In Boston and many other cities, an important proportion of the water pipes is less than 6 inches in calibre: and the ordinary size of house service tap is one-half or five-eighths of an inch.

If then water supply, as is shown under the head of *House Sewage*, is a correct measure of the ordinary sewage flow, with what propriety can any engineer construct sewers 48 inches in the clear, in the same streets and for the drainage of the same blocks of buildings supplied with 6-inch water pipes? It is true that the supply is delivered under pressure, while the discharge must be by much less head, and that storm flow must be accommodated; but with liberal allowances for these conditions, any study of water supply in most of our cities, emphatically condemns the proportions of their sewers.

In the *Analysis of Sewage* it is shown that the proportion of solid matter is rarely as high as 1 in 142, is generally assumed at 1 in 600, and is sometimes less than 1 in 1000; we see then, the abundant natural provision which exists for the prompt and complete removal of all the noxious constituents of sewage, before they can become injurious.

To accomplish this, however, it is necessary to secure a concentration of flow, in the channels of discharge, so that the scouring action of the water may be fully realized, by its force and direction, and this end can only be accomplished by a system of pipes, in its greatest perfection.

This point is very clearly illustrated, by the experiments of Mr. Blackwell, mentioned under the same head, which show the remarkable power of water currents in moving insoluble materials of great specific gravity.

For the discharge of a given body of water, a reduction of section, involves an inverse increase of velocity; and while this applies directly to the comparative efficiency of large and small branch sewers, it must also be remembered that a direct impinging current has a power greatly superior to the same current striking at an angle, or finding free vent above or on either side of any obstructing deposit. The impact of a thread of water is measured by its angle of incidence and its departing velocity, and maximum effect is obtained when these are a minimum.

The whole theory, then, of the venous or tubular system of sewerage, as subordinate to the arterial system, is based upon its complete concentration of flow, for ordinary discharge, at the time and in the places where perfect action is most desirable; and upon its more perfect adaptation to the contingencies of storm flow.

In this case, as in every other case, principle vindicates itself in the field of practice, by a mass of corroborative testimony which abounds, in a negative or affirmative form, in every system of sewers, from the remote Past to the speeding Present.

In those instances where the venous system has been ignored, and disproportionate sewers constructed, the results have amply condemned the practice, as to cost, sanitary evils, damages from floods, and annual expense of administration; built for convenience of entrance and cleaning, they have needed constant visitation and most costly excavations; of 69½ miles of sewers examined in the Surrey and Kent districts of London, the deposit was usually about 2 feet in depth, and in some cases 5 feet, reeking with foul gases, subject to explosion and choke-damp, and in an advanced state of decomposition; in New York the expense for repair and cleaning on 107 miles, was $ 45,921 in 1860, a sum which nearly doubles the entire annual sewerage administration of Brooklyn, on about the same length; as spacious reservoirs of deposit and offensive gases, they furnish food and harbor for vermin, while surcharging the atmosphere with hurtful exhalations; as peculiarly subject to overcharge in storm flow, they have been fruitful sources of public and private danger, while in strength and durability they do not compare with their more compact rivals; in point of cost, the balance against them is excessive; and from necessary roughness of line and surface, their excess in friction is not less than twenty-five per cent. on the same wet perimeter.

Pipes are therefore to be preferred as impermeable; capable of close and firm joints; smooth in surface and regular in form; of great strength; of maximum capacity for discharge; as self-cleansing; easily transported, handled, and laid: moderate in cost; and highly efficient.

Section.—In some cases of tubular systems, the oval section has been carried through all the ranges of calibre in the several groups; but, from 18 inches diameter, down to house connexions, it is doubtful whether there is any benefit realized, to balance disadvantages, in manufacture and laying. In Brooklyn the pipes of 24 inches calibre are made with an oval section of about 29 by 19 inches, which is the form lately introduced in New York, Albany, and some other places, but smaller sizes are circular, and can be more carefully made and jointed. Since, with half their section as water area, a 6 inch pipe, on a grade of 1 in 65, has a velocity of 4 feet per second, and an 18 inch pipe on a grade of 1 in 294, 3 feet per second, there can be no special advantage in improving the scouring power of the current by a reduction of the lower part of the section.

The effect of comparative perfection in surface and form for water conduits, is much more important than usually supposed. Between brick-work and earthenware unglazed pipes, a difference of 25 per cent. has been experimentally shown in favor of the latter, and between the *red pipe* and the *stoneware* pipe, experiments given in Appendix 2, page 179, London Report of 1850, show 15 to 31 per cent. difference in favor of the latter.

Perfection of form and surface is thus important, and also perfection in joints. In some of the cases of stoppages found in London and other pipes, defective joints were shown to be the arresting cause, and the difficulty which attends the perfect manufacture of earthenware pipes, has had a great deal to do with their slow adoption in some localities.

Inlets.—Pipe discharge is also seriously increased or modified, and efficiency of action promoted or hindered by the form and action of the lateral inlets. These should invariably be curved or inclined towards the line of discharge. The doctrine of "resolution of forces" is illustrated by some remarkable experiments on inlets, all of which go to show an important increase of flow in the receiving conduit, with proper inlets. In one case given Appendix 2, page 192, Report of 1850, ten inlets increase the main current 50 per cent. in velocity; in other cases the gain is stated at 300 per cent.

Materials.—The extensive use by the ancients, of earthenware pipes, many of which are still in operation, and, as we have seen at Zurich and other localities under excessive pressure, settles the question of their strength and durability. Experiments made in London, Brooklyn, and other modern cities, fully confirm this point.

But a careful discrimination must be made, between stoneware pipes, as of European manufacture, from fire-clay, and the best specimens of American manufacture, and clay pipes of coarser materials, imperfectly burned, and of irregular form and section.

On the materials of the Brooklyn pipes, Mr. Adams, in his Report of March 19, 1859, remarks:—

> "The pipe imported during the past season for use in this city, from Glasgow, Scotland, and the north of England, are entirely different from the above, being manufactured, to appearance, of a strong fire-clay, resisting a high heat, and admitting of any required thickness and consequent strength. This description of pipe is believed to be less *brittle* and less *liable to fracture* in the handling, and better suited to the purposes of a sewer than pottery-ware. It is required to be of greater thickness and to be well burnt throughout, and rendered impermeable by the glaze, which *should have become incorporated* with the material of the pipe, and not liable to separation from it. The clay for this description of pipe is found in this country, but there has been too little demand for it hitherto to encourage its manufacture. The Chelsea Pottery Works, New York, have made some trial pipes of this description which show great strength, and suitable for sewer pipes, but have not yet been brought to the requisite degree of *finish in form*,—this accuracy of form, being regarded of even more importance than a smooth interior."
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> "In packing the pipe in the press for the purpose of experimenting, many pipes were broken by the unequal bearing and cross strain induced by the effort to effect a tight joint at the ends, which in burning necessarily had become *more or less warped*, hence the pipe was badly strained before applying the hydraulic pressure. We have proved them with a head of 150 feet."

The italics in these quotations, present some of the defects to which burned pipes are liable, viz: brittleness, defective glazing, and irregular forms. The losses by breakage in handling and transportation are sometimes quite large; in those cases where the burning and glazing are imperfect, the effect of moisture gradually destroys the pipe; while an irregular or excessive heat warps the lines and joints. Earthenware pipes therefore require great care in manufacture and selection, and in this country they still fall short of the European standard of stoneware.

For reasons of this kind, and the objections to importation, preference has been given in Brooklyn and some other cities, to pipes manufactured of concrete, made from hydraulic cement mortar and beach gravel, very carefully mixed, and moulded under pressure. These pipes have great perfection in form, since the process fully admits perfect moulding, and, in common with other preparations of cement mortar, which has been thoroughly mixed, so as to avoid "drowning" on one hand, and to secure crystallization on the other, their strength is increased by age and insured by moisture. They "set" rapidly, so as to become abundantly hard in six weeks or two months: are easily fitted with branches and nipples, by any mason, in the trench; are easily transported; and are cheaper for sewerage than stoneware or brick-work. A number of miles of this kind have been laid in Brooklyn, and the entire theory of hydraulic construction, confirms the principle of their manufacture. Built on polished cast iron cores, their internal curves are exact duplicates of each other, so that perfect lines of discharge may be laid, with close socket joints, and with great smoothness of surface. In friction, durability, readiness of manufacture and cost, they may be considered the best pipe sewers in use.

Exhalations.—The use of comparatively small pipes which maintain a continuous flow, not only ensures the sanitary benefit of prompt removal for sewage matter, but also controls the air currents of these laterals, so as to carry them in the line of flow. This effect of a running stream in contracted channels, reverses the action of the system superseded, as to exhalations of noxious gases, with benefits of no ordinary value to populated districts.

Appurtenances.—These, in modern practice, consist of man-holes, lamp-holes, and street basins.

In European cities, where the street pavement is much used, access is had to the sewer by entrances from the side-walk, or some point in its vicinity, so that the work of cleaning and examination does not interfere with carriage travel; these entrances are large inclined culverts. In our cities, vertical shafts are built, from the sewer centre, generally at street intersections, or at convenient distances for cleaning.

In Brooklyn the man-holes are built of brick with an elongated working section of 4 by 3 feet at the sewer, and reduced to 2 feet diameter in the shaft, which is capped with a cast iron cover on a granite frame, or by a better pattern, on a cast iron frame at the pavement grade. At summits and other points intermediate between the man-holes, shafts of brick-work or pipes, (the latter 9 inches in diameter being the most simple,) are built to pavement grade, to be used as lamp-holes, when examinations are desired from the man-holes.

For the flow of street water into the sewers, street-basins or gullies of various forms and devices have been used in different localities. These properly belong at depressed points in gutter grades, and more particularly at street corners. They are of great use in preventing long currents of storm water from interrupting pedestrians, and damaging pavements and property, and facilitate the construction of street railways and other public improvements; they are also of great sanitary value, in preventing the common evils of gutter decomposition where gutter flow would not otherwise be properly provided for.

These basins have various forms; their office is to collect the dirt washed from the streets, while passing the storm flow into the sewers, with which they are connected by culverts or pipes; in Brooklyn pipes are used 9 inches in diameter; they are also, in modern practice, built with a trap-wall, to prevent exhalations into the streets; for the wash of cobble stone or Macadamized pavements, the use of a chamber which will contain 4 or more yards of *detritus*, and prevent its passage into the sewer, has some advantages, and is a necessity in all those cases where the sewers they connect with are not self-cleansing, but they must be cleaned at intervals, involving a costly and disagreeable operation; for this reason, where the double advantage of cleanable street pavement and self-acting sewers can be had, the basins should be built with a low discharge pipe, so as not to act as retaining wells.

An occasional want of care in the foundations of street basins and man-holes is shown by settling, which breaks joints and tubes; here the use of concrete may be strongly urged, as furnishing the most perfect base at the least cost.

Of the various details of construction for pipe sewers, as to careful lining and grading; careful bedding and tamping; the use or disuse of cradles, or grillage in soft beds; the advantages of socket joints; the position and form of inlets; the regulations proper to administration; the relative cost of various parts and kinds of work; the use and objections of tunnels in difficult cutting, and the like, much might be said of a practical nature, which we pass over, as questions rather of special application, in a discussion of general principles.

Back Drainage.—The usual system of house drainage, is by separate drains leading from the back of the lot or house through the cellar or foundation, to the street sewer. This is known as the system of *front drainage*, in distinction from a system introduced in London, to a considerable extent, by which advantage is taken of the usual arrangement of city blocks, to make the drainage entirely through the rear lots, with outlets in the side street sewers. On this point the Report of 1850, Appendix 2, page 116, thus remarks:—

> "The bulk of the drainage and the most deleterious portion is generated chiefly at the backs of the houses, and would, under the old system, be sent from thence through the house to the sewer in front, having to travel through the greatest possible space, with the least possible velocity and with the widest diffusion of the flow. In the formation of the system the disturbance of the flooring, joists, and walls of the houses, of the roadways and public streets, peculiar to this system, or of far greater amount than in the back system, to the annoyance of the inhabitants and interruption of public transit, is certainly a consideration of much weight."

On page 140 this theory is illustrated by reference to examples in practice, from which it appears, among others, that in one block of 23 houses, the lengths of front and back drains were as 1437 to 783, and the cost £131 13s. 6d. to £45 12s. 6d.; in another case of 46 houses, the lengths were as 1892 to 1143 feet, and the cost as £178 19s. 8d. to £66 5s. 2d.

Evidently, many cases occur in the arrangement of city sewerage, where this theory may be applied with great success, both as to economy in cost and as to improved action.

Conclusion.—We have attempted to group here the leading principles of correct sewerage, as illustrated by past and present experience, and put on record for the benefit of constructors. It is a subject, as will readily be observed, of too wide a range and too important a character, to be comprised in sketches of this kind, made with the outlining *porte crayon* and *currente calamo;* but if they serve to direct more careful study to their theories and to promote the advancement of educated practice, or to correct any existing evils, their purpose will have been accomplished. It may be said additionally, that it is scarcely necessary for the collaborateur to disclaim originality.

On the Construction of Wrought Iron Lattice Girders.

By Thomas Cargill, C. E.

(Continued from vol. xlvi., page 383.)

From the Lond. Civ. Eng. and Arch. Journal, Nov., 1863.

In all joints to which the definition given in our last is applicable, the actual sectional area of the plates at the line of the joint was stated to be equal to zero. Theoretically this statement is equally true for joints which are under either a tensile or compressive strain, but, practically, far more so for those under the former than under the latter condition. It is therefore apparent that the same joint, by which is meant exactly the same number and disposition of rivets and length of covering plates, is not so well adapted for one portion of a girder as for another. The joints, then, in the top and bottom members of the girder should not be identical. This also follows from the fact of the rivet holes unequally affecting the strength of the plates composing the upper and lower booms. In all girders, however, where the span is of such limited dimensions that the sectional area of the upper and lower flanches is the same, it would be unnecessary to make any difference in the jointing of the plates or angle-irons, more especially as the difference would lie on the safe side.

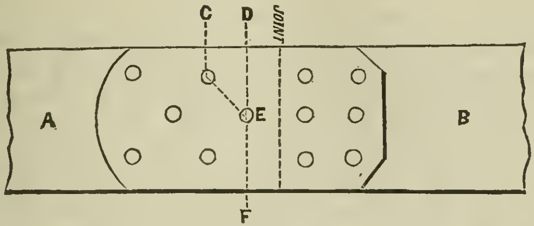
It would be impossible to construct a joint in a manner so that theoretically the value of the plates at the joint should equal that of any other portion of them, which would be equivalent to making $b\,t = (b - n\,d)\,t$, in which the minimum value of n must be equal to 1, and consequently $b = (b - d)$. If we take into consideration the additional strength and assistance afforded by the covering plates, the effect of friction, small though it may be, and look at the whole joint in the light of a firm and secure combination of parts, in many instances, practically speaking, the joint no doubt is as strong, and in some particular situations, stronger than the original plates.

To return to the subject of the disposition of the rivets and form of joints. Let us take the case of the two plates A and B in Fig. 1, which are of breadth sufficient to take three rivets across the plates. Make S and S_1 equal the net sections of the plates. S from the figure will equal $b \times t$. S_1 will equal $(b - n d) t$. If n be the number of rivet holes in the plates at the joint, and n_1 the number elsewhere where no joints occur, $\frac{\mathrm{S}}{\mathrm{S}_1} = \frac{n_1}{n}$. In any joint the very least value of n must $= 1$, and consequently the maximum value of S_1 is $\mathrm{S}_1 = \mathrm{S} - d$. The difference between the actual section S_1 and its maximum section, will altogether depend upon the manner in which the rivets are disposed in the joint. It is evident that the joint, so far as the plates were concerned, would be strongest if the section taken through the rivets in any direction across the plates were equal to $\mathrm{S} - d$. Let $\mathrm{S} - d =$ section taken along line DEF (see Fig. 1), if the section be taken along any other line, such as GEF, we obtain an increase of section, and also a loss. Let $s =$ the increase, then in order that the plate might be equally strong in any other direction as GEF, we have the following equation, putting n_1 for the number of rivet holes along the line of section:

$$(\mathrm{S} + s - n_1 d) = (\mathrm{S} - d), \text{ and } s = d\,(n_1 - 1).$$

It is not often that n can be taken $= 1$, so that a more general value for the above equation will be $s = d\,(n_1 - n)$. Let $x =$ the distance of the centre of the first rivet from the edge of the plates, and put b as before for the breadth of the plates, and d for the diameter of the rivets: then $b = 2x + 2\frac{(2x+d)}{2} = 4x + d$. The minimum value of $x = 2$ inches, and therefore, for a plate to take three rivets across its breadth, b should not $<(8+d)$. In order to determine the breadth of any plate to allow of the insertion of a certain number of rivets across it, put n, x, b, and d as before, and for a general formula $b = 2x + (n-1)\frac{(2x+d)}{2}$, from which we obtain $b = 2x - d + n\frac{(2x+d)}{2}$. The exact pitch of the rivets will depend upon the description of strain which the joint to which they belong undergoes. Let $p =$ the minimum pitch which the rivets should have in a joint exposed either to a tensile or compressive strain; and employing the same notation as above, $p^2 = \left(\frac{2x+d}{2}\right)^2 + \left(\frac{2x+d}{4}\right)^2$; and, reducing and equating, $p = \sqrt{\frac{5x(x+d)}{4} + \frac{5d^2}{16}}$. In a joint exposed solely to a tensile strain the pitch, if alternate, should be at least $= \frac{3p}{2}$, or even more, if the covers are to be well stretched and the strain properly distributed over the whole joint.

Fig. 1.

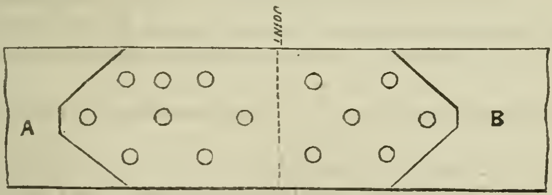


But although joints are supposed to be acted upon by either compressive or tensile strains, according to the position they occupy in the girder, and may be designed for each particular description of strain, yet it would be going too far to state positively that any portion of a girder is always subject to one kind of strain, and that it never undergoes any other. However complete our knowledge may be of the nature and effect of the different strains produced on a girder, by such loads as a weight at the centre or a uniformly distributed dead weight, it is far from perfect respecting the strains resulting from the varying action of a movable load, such as a railway train for instance. Strains of a totally different character from the normal ones are undoubtedly produced by a live load moving with even moderate velocity. As an ordinary example, take the case of a bridge consisting of two wrought iron girders, designed to carry a double line of rails on cross girders which are placed close to the bottom flanges. When a train is passing over the bridge, let the top flange of the main girder furthest from the moving load, or what might be called the girder carrying the opposite line of rails to which the train is on, be attentively observed, and it will be found to deviate in a zigzag manner from its former position, thus showing that a considerable amount of torsion is produced by the partial distribution of the moving load. A very simple method of making the above observation is as follows. Along the edge of the top flange of the girder fix three or four upright laths, or insert more, according to the length of the girder. Place the laths so that all their inner or outer edges (that is, all the edges on the same side of the laths) are out of winding. The observation may be made either with the naked eye or a telescope. The effects of torsion will be rendered visible by the edges of the laths which were previously out of winding, deviating laterally as the load advances over the bridge. A more accurate observation could be effected by having all the laths of the same size and placing them out of winding, but as the edge of the flange of a girder is never itself strictly out of winding throughout its whole length, it would not be easy to make both edges of all the laths to coincide. There would also be a vertical deviation of the tops of the laths supposing them to be all of the same height, but this would be simply the result of deflection, the effects of which can be calculated with sufficient practical accuracy.

With respect to the effects of torsion, which may be observed in the manner indicated above, three cases present themselves. The first and simplest instance is when the bridge consists of two main girders, carrying only a single line of rails. The lateral leverage of the moving load, supposing it to be placed as we have mentioned, is equally distributed upon the two main girders, and although some torsion will be produced, the principal effect is to draw the tops of the girders together. Another instance occurs when there are three main girders, the centre girder being made twice as strong as the side ones. This case is precisely similar to the former, provided that the cross-girders are not continuous. The side girder will however exhibit a greater deflection and a larger amount of torsion than the central one. If the cross-girders are continuous, the moving load takes effect upon the side girder belonging to the other line of rails. Let S equal strain on central girder produced by a moving load, and S_1 that produced by the same load on the side girder furthest from the load, let e equal gauge of railway, then $\frac{S}{S_1} = \frac{3 \times e}{2} \times \frac{2}{e} = 3$. The third case is the one described before, in which the double line is carried by two main girders. The resistance of the side girder furthest from the moving load will in this instance be greater than when there are three main girders, although the leverage remains the same.

In Fig. 1, the joint as shown belonging to the plate B is evidently a more economical one, so far as the wrappers are regarded, than that of the plate A. Let l, l_1 = the half lengths of the wrappers for the plates A, B, respectively, $l = x + np$, $l_1 = 2x + (n-1)p$, and $l - l_1 = p - x$. The difference however would be much greater than that given in the equation, for the value of p would not be the same for both tensile and compressive joints; for the latter of which the joint at plate B is best suited. There is also a great dfference in the value of the net sections, which are in the proportion of $\frac{n}{n_1}$, or as 3 : 1. In the case of B, the net section would be considered nearly equal to that of the plate, especially if the riveting were performed by machinery. The same description of joint is represented in Fig. 2, with a slight modification in the shape and length of the wrappers. A difference may be also here observed in the disposition of the rivets next the joint; in the plate B there are two rivets equally distant from the joint, while there is only one in the plate A. If a strain sufficient to cause fracture were applied, the ultimate net sections, or the line of fracture, across the plates would be the same, the pitch of the rivets being similar. In a joint exposed solely to a compressive force the rivets might be arranged in either manner, although the arrangement in plate B would be the preferable one. The contrary will be the case where the strain is of a tensile nature, in consequence of the shearing force induced in a longitudinal direction upon the plates. Let H = the tendency to draw the rivets nearest the joint, through the plates in the above direction, n the number of rivets equally distant from the joint, c the distance; then, the diameter and number of the rivets being the same $H : : \frac{1}{c}$. It will be seen that, in the plates A and B in Fig. 2 since the value of n and n_1 are different, the shearing forces are as 1 : 2, or more correctly speaking, the tendency to bring the shearing force into action is as above.

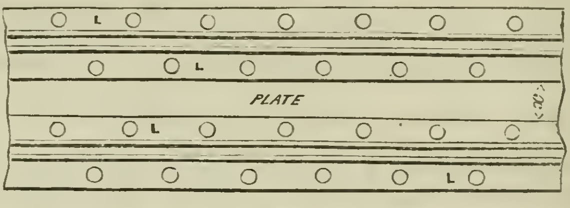
Fig. 2.



It would be needless repetition to give any more examples of joints of simple plates, in which the difference would consist merely in the dimensions of the plates. Whatever the breadth may be, the formula $b = \frac{(2x-d)+n(2x+d)}{2}$ will always hold, and the number of rivets to be inserted in the breadth of the plate will equal n, and $n = \frac{2(b-x)+d}{(2x+d)}$. The same principles will equally apply respecting the disposition of the rivets so as to distribute the strain uniformly over the wrappers and plates, and the judgment and experience of the designer must supply those trifling variations and modifications which inseparably accompany the application of general rules to particular examples.

We now will proceed to the consideration of a joint which is of constant occurrence in practice—viz: the jointing together of two plates forming the portion of a boom of a double-lattice girder. In order however to distinctly point out the effect which the riveting of the longitudinal angle-irons to the plates has upon the disposition of the rivets at the joint, a plan of one of the plates is shown in Fig. 3, where no joint occurs. The figure represents a portion of the inside of one of the plates, with four, or two pairs of angle-irons united to it by uniform longitudinal rows of rivets, equally pitched from centre to centre. Between each pair of angle-irons are riveted the bars composing the web of the girder. The sectional ends of the bars as well as the connecting rivets are not shown in the figure, to avoid confusion, besides they are not required for our present purpose. To obtain the advantage of the full breadth of the plates, and to keep the distance between each pair of angle-irons as great as possible, the first angle-iron of each pair is always placed flush with the edge of the plate. It is obvious from this arrangement, that the four longitudinal rows of rivets shown in the figure are at once determined, or what amounts to the same, the position of four rivets in the breadth of the plates is fixed independently of any other joints or connexions which it may be necessary to make. Let $l =$ the length of the side of the angle-irons which is riveted to the plates, then the distance of the centres of the first row of rivets from the edges of the plates will $= \frac{l}{2}$, l being measured from out to out; that is, if l_1 be the length of the inside of the vertical side of the angle-irons, and t the thickness of the horizontal sides, $l = l_1 + t$. Similarly, putting m for the maximum thickness of the web, which will correspond to the space between each pair of angle irons, we shall have the distance between the centres of the first and second rows of rivets equal to $l + m$, or the distance of the centres of the second rows from the edges of the plates equal to $(2l + m) - \frac{l}{2} = \frac{3l}{2} + m$. The value of m will in general be the sum of the thicknesses of the two thickest ties and struts in the web. Its exact value will depend upon the manner in which the

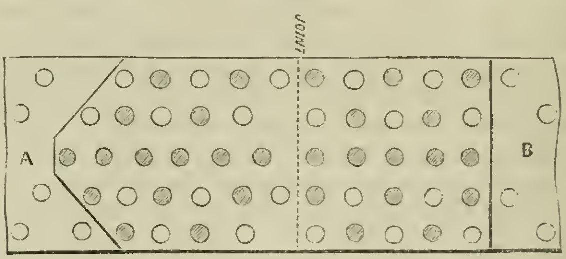
Fig. 3.



struts are riveted in between the angle-irons. The thickness of any strut or tie is a maximum at the abutment, and a minimum at the centre of the girder; it therefore becomes necessary, in order to preserve the distance between each pair of angle-irons constant, to insert filling or packing pieces between the ends of the bars and the angle-irons. If t be the thickness of the web at any point, p the thickness of the corresponding filling piece or washer, $p \propto \frac{1}{t}$. The limits of p will range between $p = 0$ and $p = m$. A very large proportion of the packing pieces might be got rid of if the second angle-irons of each pair, instead of being maintained at a constant distance from the first, were riveted to the plates in a line gradually converging towards the centre of the girder so as to regularly diminish the space between them. The practical inconveniences and chances of error attending the punching of the holes in the plates in the above manner would not justify its adoption of girders in ordinary spans, where the saving of material would be inconsiderable.

As the longitudinal angle-irons connecting the booms and webs must be attached to the plates by regular rows of rivets in a manner similar to that shown in Fig. 3, either with an alternate pitch or otherwise, it is practically impossible to design the joints for the plates in the best and most efficient manner, as would be the case were there no angle-irons riveted to them. In fact, the rivets required for this purpose virtually determine the description of joint which we are able to employ. The result of this limitation is, that the joints in the flange plates of ordinary girder work are nearly always better adapted to resist the strain on the top boom than on the bottom, for which they are most required. Very little room is left for the exercise of the judgment of the designer, and not much more can be done than to take care to insert a sufficient number of rivets to hold the plates and make as good a joint as possible under the circumstances. The truth of this will be perceived on examining Fig. 4, which represents the reverse sides of the plan in Fig. 3, with the addition of a joint and wrapper, and in which the plain circles are the rivets inserted through the plates and angle-irons, and the shaded ones those necessary to complete the joint. It will be found, on observing girder work, that if $p =$ the pitch of the rivets such as shown in Fig. 3, as a general rule the pitch of the rivets in the joints will equal $\frac{p}{2}$. Two arrangements of the rivets for the joint are given in Fig. 4, the one on the right on plate B being very frequently employed, although it is not so well suited for the lower boom of a girder as the other on the left on plate A. Let S = section of plates in a straight line across the plates, and C any increase of section arising from S not being in a straight line, then (S—nd) will be the available amount of metal in the plates wherever S is taken in one continuous straight line. In plate B the minimum value of n is 5, in plate A with section S its minimum is 2 and maximum 4. It has, the same as in plate B, a maximum value of 5, but then the section is no longer S, but (s+c). For the joints in the lower flange of a girder, where the tensile strain tends to separate the plates or pull the joint asunder, there is a slight advantage gained in disposing the nearest the joint, as shown on plate A. Let p = the alternate pitch of the rivets, and let n, n_1, n_2 = the number of rivets nearest the joint in plate A, which are respectively at equal distances from it, similarly put m for the number nearest the joint in plate B. The resistance of the former plate in comparison with the latter, against the calling into play of a longitudinal shearing force, will be as $\frac{p}{2}(n+n_1+3n_2) : : \frac{p}{2} \times m$; substituting the values for n, n_1, n_2 and m we have the ratio as $\frac{9}{2} : : \frac{5}{2}$. Whenever the pitch of the rivets in the plates and angle-irons is not alternate, the simplest joint is made by inserting a rivet in each row halfway between the others, and adding as many additional rows as the breadth of the plate will allow, until the proper number is put in. The joint will then be precisely similar to the one in plate B, in Fig. 4. In large girders, and wherever the importance of the work demanded it, an interruption in the regular punching of a portion of the plates near their ends, and of a corresponding length of some of the angle-irons, must take place, otherwise the joints cannot be designed and the rivets inserted in them to the best advantage, particularly in those joints which, from their position in the girder, would be exposed to a tensile strain. It is manifest, under the most favorable circumstances, that the jointing of a combination of plates and angle-irons cannot be considered in the light of a simple jointing of two plates, and that some modification and departure from the most efficient disposition of the joint must ensue, although in important examples the correct principles may be adhered to with sufficient practical fidelity.

Fig. 4.



It should be borne in mind in designing joints, that the value of n being constant, the length of the covering plates varies directly as p, and therefore care should be taken not to spread the rivets too much. On the other hand, the rivets should not be huddled together, and a more than necessary amount of weakness produced in the plates, for the sake of effecting a small saving of material in the wrappers. It might be alleged that the thickness of each wrapper need not be greater than half that of the thinner plate to be jointed, or, what amounts to the same, that the sum of the thicknesses of the wrappers should equal the thickness of the thinner plate. Theoretically this would give a sufficient amount of section at the line of the joint, provided the strains to which the joint was exposed affected it perfectly uniformly. But practically we know this not to be the case. The disposition of the rivets in the joints, the manner of loading, and a variety of other contingencies, frequently cause joints to be more strained in one part than another, and the same occurs in the wrappers. It will be far safer to make each wrapper at least equal in thickness to that of the thinner plate, or, what is better, to make the sum of the thicknesses of the wrappers equal that of the two plates. Put t for the thickness of each wrapper and t_1 t_2 for that respectively of each plate; then, for each wrapper to have the same thickness, $t = \frac{t_1 + t_2}{2}$. On referring to Fig. 3, it will be seen that the wrapper of the inside of the plates will only occupy a breadth of the plates equal to x, and will not extend the whole breadth of the plates, like the outside wrapper in Fig. 4, but fit in between the angle-irons, which supply the remaining portion of the wrapper and act as such. In many instances, were it considered worth the omission, this small inside wrapper might be dispensed with. To obtain the maximum theoretical efficiency in a joint, the value of n should be infinitely great, and that of d infinitely small. In small girders, as before mentioned, in order to save trouble and inconvenience in punching the rivet-holes, the joints in the upper and lower flanges are generally made identical, but in girders of large span they should be differently proportioned. Let n and n_1 equal respectively the number of rivets required in the half joint of a plate subject to tensile and compressive strains. Employing our usual notation, and supposing the same constant employed for both, we have $\frac{n \times \pi d}{4} = n_1 \times t$, and $\frac{n}{n_1} :: \frac{4 \times t}{\pi \times d}$. As a higher constant might with perfect safety be used, we may take into our calculations the increase of strength afforded by the friction between the plates and wrappers, more especially if the latter be of considerable size, and also of the rivets themselves. We shall then find $\frac{n}{n_1} :: \frac{2t}{d}$, the value of n being increased in the proportion of $\frac{\pi}{2} : 1$, supposing the diameter of the rivets to be the same size.

Whatever may be the peculiar form or section of iron to be jointed, the same principles will apply, although a little artifice may be required to meet the exigencies of particular examples. In Fig. 5 a plan of the joint of one of the angle-irons in the flange of a girder is shown, AB being the centre line dividing the flange longitudinally. One of the wrappers consists of another piece of angle-iron, fitting into the main angle-iron, which answers the double purpose of one horizontal and one vertical wrapper. The other vertical wrapper is supplied by a piece of flat iron placed as shown in the figure. The other horizontal side of the joint is covered by the main plate of the flange, and therefore no second wrapper is required in addition. This arrangement is better represented in section, in Fig. 5*a*, the line AB dividing the flange vertically at right angles to the line AB in Fig. 5. The length of the horizontal and vertical sides of the angle-iron wrappers should be such that they should not project beyond those of the angle-irons to be jointed, but be flush with their edges, as in the section referred to. To give it the same sectional area, it must thus necessarily have a greater thickness than the main angle-irons. Let A = area of main angle-irons, and A_1 that of the wrapper, then, as the value of n will be the same in both instances, $A_1 = A$. Put l and l_1 for the length of the vertical and horizontal sides respectively of the main angle-irons measured from out to out, and t for the uniform thickness, also let t_1 be the required thickness of the wrapper. A will then = $(l + l — t)\,t$, and $A = [(l—t)+l_1—(t+t_1)]t_1$, and we shall have the equation $t^2—t_1(l+l_1—2t) = —t(l+l_1—t)$,* from which, solving for t_1, we obtain

Fig. 5.

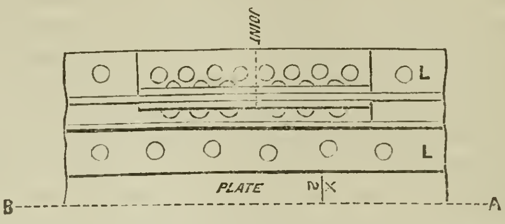
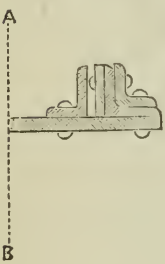


Fig. 5 *a*.



$$t_1 = \pm\sqrt{\frac{[(l+l_1)^2—8t(l+l_1—t)]}{2}} + \frac{(l+l_1—2t)}{2}$$

For example, let the dimensions of the main angle-irons be 4½″ × 3″ × ½″, then the value of t_1 which will give an equal sectional area to the wrapper will be $t_1 = 595$, or practically $\frac{9}{16}$. The sectional area of the flat vertical plate acting as a wrapper will equal $(l—n)t$.

In making the drawings for the girder, care should be taken to show all the joints in the positions they are intended to occupy. If a mere general design and description be given, they will frequently be found to be placed in too close proximity to one another. Unless where unavoidable no joint should be at a less distance from another than three feet. On referring to Fig. 5, it is manifest that the joints in the angle-irons must be so arranged that they should not occur at places where the flanges are intersected by the bars of the web, otherwise the second vertical wrapper would have to be omitted. Independently of this consideration, it would be bad construction to place a joint in the angle-irons to which the bars of the web are attached, at the point where that attachment is made, and consequently the greatest amount of local strain induced. The same reasoning applies to the joint of the main plates of the flanges, but with much less force. since the strains are not received directly by them, but are transmitted to them by the longitudinal angle-irons, which also serve to diffuse them more uniformly over their surface. It would be well to cause the rivets placed vertically in the angle-iron joints and wrappers to alternate with those in the horizontal position, but this is not of so much importance in sections of rolled iron similar in form to that of angle or tee iron, as in flat plates or in plain bar sections. The effect of a strain to cause fracture through a line of rivet-holes in a plate, the surface of which was all in the same plane, would be greater than if the same amount of metal in the plate were rolled into a form having equal sections distributed in two planes at right-angles to one another, with half the rivet-holes in each plane.

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* This equation resolves itself into one belonging to the general class $x^2 - ax + b = 0$, one root being rejected by the nature of the problem.

(To be Continued.)

Observations on Foundations of Bridges, &c By THOMAS PAGE, M. Inst. C.E.,F.G.S.,F.R.G.S.

[Paper read to the British Association, 1863.]

From the Lond. Civ. Eng. and Arch. Journal, Nov., 1863.

It is not proposed in the following observations to give a history of foundations, but only to refer to the system pursued by the author in the various works he has executed under the water level without the use of coffer-dams, which, whatever may be their advantage, are a most expensive means of construction. It may be quoting an extreme case to refer to the repairs of the Old Westminster Bridge in the year 1838, where, in a contract for the repairs of that structure amounting to £49,500, the permanent work amounted to £14,500 and the coffer-dams to execute that work to £35,000. But viewing the subject of coffer-dams as a whole it possesses but little engineering merit, compared with its expense, as a means of construction in foundations.

The author's experience on this subject embraces the Thames Tunnel, of which he was Acting Engineer under Sir Isambard Brunel, from 1836 to its completion, a period which embraced the most difficult part of the construction of that great work (being nearly half its entire length) and also the formation of the shaft on the Middlesex side; it also includes the works of the Victoria Bridge, and the Albert Bridge, over the Thames at Windsor ; the Chelsea Suspension Bridge ; the New Westminster Bridge ; and his design for the foundation of the Albert Pier at Greenock, carried into execution by Messrs. Bell and Miller, Civil Engineers, of Glasgow.

Principles of a Foundation.—A foundation may be described as that part of a structure which resists the weight of the superstructure ; and it is evident that the higher the horizontal plane of the resisting mass, the less is the weight of the superstructure upon it, and the better adapted is the foundation to resist its pressure. For instance, if, to follow out the injunction, "go deep enough to obtain a good foundation for the piers of a bridge," the resisting surface is 30 *feet below* the low-water line, while in another case the resisting surface is *level* with the low-water line, it is evident there is a pressure due to 30 feet in height of the material of the piers greater upon one foundation than the other. If this principle were always acted on—viz : to so construct the foundations that the plane of resistance should be at a high level—the difficulty of founding in deep water would be considerably reduced.

System of the Author as practised at the New Westminster Bridge.—The system which the author has pursued in the construction of the four bridges over the Thames before referred to, may be thus described. The positions of the piers and abutments having been accurately defined, bearing piles are driven deep into the ground over the area of each pier and abutment, at a distance of 3 feet between each pile. All the piles are driven to the same test, so that every pile is capable of supporting the same amount of load.

This result is not dependent on the length of the piles, for some piles attain the resistance to the test sooner than others, depending on the state of the stratum through which they are driven. In many bridges the piles have been specified to be driven to a specified length, but this condition did not secure equal degrees of resistance, which can only be attained by driving the piles to the same test.

The piles being driven, their tops are cut off at the low-water line of spring tides ; and on the line of the perimeter of the pier cast iron piles are driven at distances of 5 feet 2½ inches apart, centre to centre, in which are grooves for the cast iron plates or sheeting piles. In Westminster Bridge these sheeting piles or ribbed plates were 16 feet in length and 4½ feet in breadth, and were driven down until the top flanges were 6 *feet below the low-water line ;* the 6 feet in height from those flanges up to the low water-line, was formed of granite panels 2 feet thick instead of the cast iron piles, so that no iron but the main piles appears in the face of the piers to a depth of 6 feet below the low water-line. The timber piles are then tied to each other and to the iron piles by wrought iron bars, and two tie-bolts were inserted at the head of every sheeting pile 7 feet below low-water, and a tie-bolt to every main pile at the *low-water level.*

The bed of the river was then cleared of all loose materials down to the hard gravel, and in many cases down to the clay, and the spaces between the piles were filled with concrete made with Portland cement and gravel, which concrete was deposited from boxes (opening with flaps) on the bed of the river, and which concrete soon became as hard as a rock. All the upper surface of the pier was then brought to a true level for the reception of the great base course of granite, and on this granite floor the pier was carried up with brick-work and granite.

To avoid the scour round the piers, which is felt at all the bridges of the metropolis in every case where any set of the current strikes the pier in an oblique direction, a base of Portland cement concrete was deposited round the piers of the New Westminster Bridge.

On the Foundations of the Chelsea Bridge.—In the Chelsea Bridge the casing of the piers was of cast iron, the main piles being 9 feet apart (central distance) and the sheeting piles and plates being 7 feet 2 inches in width. As these were carried to a level of 7 feet above Trinity high-water, and to a depth of 41 feet below, there was a surface of iron 48 feet in height.

The proceedings in arranging and driving the timber bearing piles, tying them across by wrought iron bars, dredging the bed of the river, and filling all the spaces round the piles with concrete, were similar to the proceedings at Westminster Bridge, which followed Chelsea Bridge in point of time.

Of the Greenock Pier.—The introduction of granite slabs or panels at Westminster Bridge led to their adoption at the new pier of Greenock, the drawings for which, prepared by Messrs. Bell & Miller of Glasgow, Civil Engineers, were referred to me, at their suggestion, by Mr. Jas. F. Grieve, the Provost of Greenock. The alteration I suggested in the face work of the pier, instead of building it in courses on a principle patented by Messrs. Bell and Miller, was to form it by large granite slabs or panels each 8 feet high by 7 feet wide and nearly 2 feet thick, between cast iron piles; the front and back flanges of the iron piles were arranged for the granite panels to slip down between them, and as these panels were each 8 feet in height, an enduring face 16 feet in height was obtained at a considerable saving of cost, in the most simple and comparatively inexpensive manner.

The granite used at Greenock Pier is of fine reddish granite from a spacious quarry in the Isle of Mull, on the Estates of his Grace the Duke of Argyle, worked by the Isle of Mull Granite Company. This granite was also partly used at Westminster Bridge, with the Cheesewring, Haytor, and Par granites, and is admirably adapted for hydraulic works on a great scale.

Of the Piling of Old London Bridge.—It is worthy of remark that the bearing piles of Old London Bridge were cut off on the level of the low-water line, but it was without reference to this circumstance that the bearing piles of the Chelsea Bridge and those of the New Westminster Bridge were arranged to be cut at the level of the low-water line.

General Observations.—It is due to the British Association to state the reasons why the author prefers the system he has brought under the notice of the Section to the systems of foundations by cylinders of iron, filled with brick-work or concrete, which has been so generally acted upon during the last fifteen years. He considers it important that the foundation of each pier should be one undivided structure; that it should not be broken into separate parts, as it is in cases where cylinders are used, and that, besides the resistance due to the horizontal area of the foundation, it should embrace the additional resistance afforded by the friction due to the vertical surfaces of the piles; and thus, short of founding on rock itself, it would present the most solid resisting mass that could be formed.

The facility and rapidity with which such foundation can be formed with a surface of granite or other durable stone, recommends itself for breakwaters in deep water in preference to the system of forming a slope with a large expenditure of material and labor, and it is to be considered that a vertical face is less dangerous to vessels approaching a breakwater in rough weather, as against it the sea would only dash without breaking, while on the slope the waves become *breakers ;* and many instances have been known of ships being lost upon the slopes. For such a Harbor as Dover, which is built of concrete blocks faced with granite, all being set by divers, the formation of a granite face in panels, and the core filled in with concrete *en masse*, deposited in the same manner continuously as at Westminster Bridge, would secure a rapid and economical construction.

The application of this system of construction to harbors is a subject of great interest and importance at the present time, both for expedition in completing the works, and for economy, but as this paper is especially devoted to *foundations*, any further observations must be reserved for another opportunity.

MECHANICS, PHYSICS, AND CHEMISTRY.

An Investigation of Plane Water-Lines for Ships.

By Prof. RANKINE.

From the London Athenæum, Sept., 1863.

This paper contains an abstract of a mathematical investigation which has been communicated in detail to the Royal Society. By the term "Plane Water-Line" is meant one of those curves which a particle of liquid describes in flowing past a solid body, when such a flow takes place in plane layers. Such curves are suitable for the water-lines of a ship; for, during the motion of a well-formed ship, the vertical displacements of the particles of water are small, compared with the dimensions of the ship ; so that the assumption that the flow takes place in plane layers, though not absolutely true, is sufficiently near the truth for practical purposes. The author refers to the researches of Prof. Stokes (*Cambr. Trans.* 1842) "On the Steady Motion of an Incompressible Fluid," and of Prof. William Thomson (made in 1858, but not yet published), as containing the demonstration of the general principles of the flow of a liquid past a solid body. Every figure of a solid past which a liquid is capable of flowing smoothly, generates an endless series of water-lines which become sharper in their forms as they are more distant from the primitive water-line of the solid. The only exact water-lines whose forms have hitherto been completely investigated, are those generated by the cylinder, in two dimensions, and by the sphere, in three dimensions. In addition to what is already known of those lines, the author points out that when a cylinder moves through still-water, the orbits of each particle of water is one loop of an elastic curve. The profile of waves has been used with success in practice as water-lines for ships, first, by Mr. Scott Russell (for the explanation of whose system the author refers to the *Transactions* of the Institution of Naval Architects for 1860-61-62), and afterwards by others. As to the frictional resistance of vessels having such lines, the author refers to his own papers; one read to the British Association, in 1861, and printed in various engineering journals, and another read to the Royal Society, in 1862, and printed in the *Philosophical Transactions.* The author proceeds to investigate and explain the properties of a class of water-lines, comprising an endless variety of forms and proportions. In each series of such lines, the primitive water-line is a particular sort of oval, characterized by this property, that the ordinate at any point of the oval is proportional to the angle between two lines drawn from that point to two foci. Ovals of this class differ from ellipses, in being considerably fuller at the ends and flatter at the sides. The length of the oval may bear any proportion to its breadth, from equality (when the oval becomes a circle) to infinity. Each oval generates an endless series of water-lines, which become sharper in figure as they are further from the oval. In each of those derived lines, the excess of the ordinate at a given point above a certain minimum value, is proportional to the angle between a pair of lines drawn from that point to the two foci. There is thus an endless series of ovals, each generating an endless series of water-lines; and amongst those figures, a continuous or "fair" curve can always be found, combining any proportion of length to breadth from equality to infinity, with any degree of fulness or fineness of entrance, from absolute bluffness to a knife-edge. The lines thus obtained present striking likenesses to those at which naval architects have arrived through practical experience; and every successful model in existing vessels can be closely imitated by means of them, from a Dutch galliot to a racing-boat. Any series of water-lines, including the primitive oval, are easily and quickly constructed with the ruler and compasses. The following curves, traversing certain important points in the water-lines, are exactly similar for all water-lines of this class, and are easily and quickly constructed with the compasses. One is a hyperbola, which traverses all the points at which the motion of the particles, in still-water, is at right angles to the water-lines. The other consists of the two branches of a curve of the fourth order. One of those branches traverses a series of points, at each of which the velocity of gliding of the particles of water along the water-line is less than at any other point on the same water-line. The other branch traverses a series of points, at each of which the velocity of gliding is greater than at any other point on the same water-line. The transverse axis of co-ordinates, so far as it lies within this branch, traverses a series of points of minimum velocity of gliding; from its intersection with the same branch onwards, it traverses a series of points of maximum velocity of gliding. Every water-line, complete from bow to stern, which passes within the point of intersection of the same branch with the transverse axis has three points of minimum and two of maximum velocity of gliding; while every water-line which passes through or beyond that point has only two points of minimum and one of maximum velocity of gliding. Hence the latter class of lines causes less commotion in the water than the former. On the water-line which traverses the said point itself, the velocity of gliding changes more gradually than on any other water-line having the same proportion of length to breadth. Water-lines possessing this character can be constructed with any proportion of length to breadth, from $\sqrt{3}$ (which gives an oval) to infinity. The finer of those lines are found to be nearly approximated to by wave-lines, but are less hollow at the bow than wave-lines are. The author shows how horizontal water-lines at the bow, drawn according to this system, may be combined with vertical plane lines of motion for the water at the stern, if desired by the naval architect. In this, as in every system of water-lines, a certain relation (according to a principle first pointed out by Mr. Scott Russell) must be preserved between the form and dimensions of the bow and the maximum speed of the ship, in order that the appreciable resistance may be wholly frictional and proportional to the square of the velocity (as the experimental researches of Mr. J. R. Napier and the author have shown it to be in well-formed ships), and may not be augmented by terms increasing as the fourth and higher powers of the velocity, through the action of vertical disturbances of the water.

A discussion ensued, in which Messrs. J. Scott Russell, T. Webster, J. R. Napier, and Prof. Pole took part.

Proceedings British Association.

For the Journal of the Franklin Institute.

Atmospheric Pressure as a Mechanical Power.

In a recent article in the *Journal*, on the use of Air as a motive power in cities, reference is made to various processes for producing a vacuum under a piston, and the inference drawn that they are all too costly, too slow, or, in other respects unsuited for obtaining a popular working force from the pressure of the atmosphere. Of others not mentioned, there is one that may be worth suggesting.

I believe there are no forces but what are designed to be employed in the arts: that all, known or unknown, are to be brought into the service of man; that nature not only provides them, but in her applications of them, is their best expositor, and that whatever she effects for herself with one, we can produce it by the same or analogous results for ourselves. On the present occasion, I would refer to the means by which she disturbs the equilibrium of the atmosphere in the instantaneous condensation of vapor into rain, and the resultant ærial squalls and commotion that prevail in temperate and become so terrific in tropical latitudes. Here we perceive she is doing the very thing we want to do—employing the weight of the atmosphere as an active motive power—neutralizing its influence on one spot and accumulating it on another, by forming a vacuum as it were in a cylinder whose sides pressed together, perform the part of a piston with us. Not a day, probably not an hour, passes, in which these phenomena do not occur in one part of the earth or another.

The fall of rain does not appear to depend on temperature alone.

since it comes down night and day in all weathers; in summer as in winter, and in the warmest climes in actual torrents. The influence of electricity over atmospheric phenomena is universally admitted, and by some, if not by all, meteorologists, it is deemed the *immediate* cause of rain. That tempests, hurricanes, tornadoes, &c., arise from the vacuum thus suddenly produced, is still more generally conceded. They are commonly accompanied with thunder and of brief duration, subsiding as the voids become filled and the equilibrium restored. In some regions where thunder is little known, it is said, there is little rain. Now, why may not artificial clouds of vapor—of low steam in a cylinder—be as quickly collapsed into water by the same means, and vacuities produced to serve our purposes as nature produces them to serve her own? I suppose they may. If some element should be wanting in the process it is our part to supply it. If *air*, for example, be found essential, a limited quantity might be admitted. If the condensation should not be effected as rapidly as anticipated, the area of the piston might be proportionally increased, and so with regard to other points. Electricity, as a science, is in its infancy. It has opened some of Nature's secrets, but they are few compared to those yet to be explained. Among them is the part it plays in the phenomenon in question. If, as is asserted, it causes the aqueous corpuscles suspended in the atmosphere to coalesce and descend, sometimes in drizzle as through the finest cullender, at others in showers of large drops, and again in sheets as if from the bursting of water-spouts, we ought to be able to bring out kindred results by it to establish the theory.

But without reference to any theory, the object is to learn how nature effects those sudden collapsions of vapor. Be the process what it may, mechanical or chemical, or a combination of both—whether clouds surcharged with moisture are discharged by compressure, somewhat as we squeeze a saturated sponge or wring out wet linen, or whether it be, as suspected, an electric talisman that neutralizes the power that keeps the aqueous globulus apart and holds them in suspension; or anything else—when thoroughly known, we can certainly master it sufficiently to work out on smaller scales the same result. If, when developed, it should not be found preferable to processes we have, it is still worth finding out, were it but to determine that fact. But if, on the other hand, it should turn out much better than the best of them—simpler, cheaper, and more prompt, and there should be no obstacles to its general adoption—the benefits it would confer on our species for all time to come, would surpass those of other forces combined, steam included. It would meet almost every call for labor, for domestic and manufacturing dredgery in-doors and out. In a word, I can hardly imagine a nobler acquisition in practical science than the derivation of a general working power from the pressure of the atmosphere. A stronger stimulus to solve a great problem there cannot be.

It is probably in some such indirect way as this that electricity, in one or more of its forms, is to become, if ever, a popular motor, not directly as has so often been attempted and as often failed. Let us use it as Nature does and we are sure to succeed.

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On the Proportions of Ships of least Skin Resistance for a given Speed and Displacement. By Prof. RANKINE.

From the London Athenæum, Sept., 1863.

The author referred to a previous paper which he had read to the British Association in 1861, and in which he had stated the results of a theoretical investigation of the "skin-resistance" of ships, and verified those results by a comparison with those of experiments. In the course of that paper he had stated, that the theory gives, for the proportion of length to breadth which produces least skin-resistance with a given displacement and speed, that of *seven* to *one*, nearly. This is the case when the figures and proportions of the cross-sections are given, so that the draft of water bears a fixed proportion to the breadth. But, when the draft of water has a fixed absolute value, the theory gives a somewhat different result; for the proportion of length to breadth which produces the least skin-resistance is found to increase as the draft of water becomes shallower.

Proceedings British Association.

For the Journal of the Franklin Institute.

On the Integral of $\frac{dz^2}{dx^2}=\frac{8w}{\mathrm{R}}z^{\frac{3}{2}}+\mathrm{c}$.

By DE VOLSON WOOD. Prof. of Civ. Eng. Univ. of Mich.

In an article on "*Beams of Uniform Strength*," published in Vol. xlii, third series, page 110 of this *Journal*, will be found the equation at the head of this article in which c is the constant of integration. I raised the query whether it could be integrated in finite terms. Upon a recent examination of it, I find that it may be integrated by means of the higher transcendentals, in the following way:

We have $$dx=\frac{dz}{\sqrt{\mathrm{c}+\frac{8w}{\mathrm{R}}z^{\frac{3}{2}}}}=\sqrt{\frac{\mathrm{R}}{8w}}\;\frac{dz}{\sqrt{\mathrm{c}_1{}^2+z^{\frac{3}{2}}}}\quad . \qquad . \quad (1)$$

where $\mathrm{c}_1{}^2=\frac{\mathrm{R\,C}}{8w}$.

Let $z^{\frac{3}{2}}=-u^2$ and the equation becomes

$$dx=-\frac{2}{3\mathrm{c}_1}\sqrt{\frac{\mathrm{R}}{8w}}\;\frac{u^{\frac{1}{3}}\,du}{\sqrt{1-\frac{u^2}{\mathrm{c}_1{}^2}}}\quad . \qquad . \qquad . \quad (2)$$

Now let $$\frac{u}{\mathrm{c}_1}=\left(1-y^2\right)^{\frac{3}{2}}\quad\therefore\quad u^{\frac{1}{3}}=\mathrm{c}_1^{\frac{1}{3}}\left(1-y^2\right)^{\frac{1}{2}}$$

$$du=\tfrac{3}{2}(1-y^2)^{\frac{1}{2}}dy$$

(This y is not the depth of the beam as used in the problem.)

These substituted in the preceding equation gives

$$dx = -c^{\frac{1}{6}} \frac{\sqrt{R}}{2w} \quad \frac{1-y^2}{\sqrt{3-3y^2+y^4}} dy \quad . \quad . \quad (3)$$

As the solution of this, in finite form, involves the use of *elliptic functions*—a branch of mathematics extensively studied by comparatively few students—I shall only indicate the steps.

Equation (3) is in a convenient form to put in ordinary form for elliptic functions. See *Le Gendre's Elliptique Fonctions*, page 9, 6th paragraph. The roots of the denominator are all imaginary; hence it comes under the first case. This case in the form of Equation (3) is given on page 55 of Le Gendre.

The form is

$$x = \int \frac{(f+gy^2)\,dy}{\sqrt{a^2+2aby^2\cos.\theta+b^2y^4}} \quad . \quad . \quad . \quad . \quad (4)$$

The integral of which is given on page 56, and is

$$= \frac{fb+ga}{b\sqrt{ab}} \quad \mathrm{F}_{(c,\varphi)} - \frac{2ga}{b\sqrt{ab}} \mathrm{E}_{(c,\varphi)} \quad . \quad . \quad . \quad (5)$$

Equation (3) compared with (5) gives

$$f=1 \qquad a^2=3 \qquad \cos.\theta = -\tfrac{1}{2}\sqrt{3}$$

$$g=-1 \quad b^2=1 \quad \therefore \quad \theta=120°,$$

which substituted in (5) gives

$$x = -\left(\frac{Rc}{8w}\right)^{\frac{1}{6}} \left(\frac{R}{2w}\right)^{\frac{1}{2}} \left[\frac{1-\sqrt{3}}{\sqrt[4]{3}} \mathrm{F}_{(c,\varphi)} + 2\sqrt[4]{3}\, \mathrm{E}_{(c,\varphi)}\right] \quad (6)$$

We also have $c = \sin.\tfrac{1}{2}\varphi$.

$$\cos.^2\varphi = \frac{-by^2 - a\cos.\theta + \sqrt{a^2+2aby^2\cos.\theta+b^2y^4}}{2a\sin.\frac{1}{2}\theta} \quad . \quad . \quad (7)$$

$$\therefore \quad \varphi = 0 \text{ for } y = 0$$

Equation (6) gives $x = 0$ for $\varphi = 0$ as it showed, since x and y are both zero at the origin.

To construct the curve we may assume y, and by the relations given above, find u and $z^{\frac{1}{2}}$, which last value will be the ordinate. To find the corresponding abscissa, substitute the assumed value of y in equation (7) and find φ, and then Table IX, Vol. II, Le Gendre, will give $\mathrm{F}_{(c,\phi)}$ and $\mathrm{E}_{(c,\phi)}$ which in equation (6) will give φ.

It is difficult to find c, the first constant of integration, because its value is contained in a transcendental function.

This solution induces me to make a remark upon the integration of expressions.

We know that the cases which will admit of integration in finite form are comparatively few. Any known function may be differentiated (or disintegrated), and the result is *typical* of the infinitessimal-elements which result from the function.

But it is not *generally* possible to pass from the differential to the integral, because we do not know the *form* of the function which will give the *form* of the given differential. It is generally necessary to have the function (and this is known first) and the typical infinitessimal-element before we can deduce rules for passing from the latter to the former. Thus we know how to integrate sin. $x\,dx$, because we know the form of the function from which it is derived, viz: — cos. x; but we cannot integrate log. $x\,dx$ (in finite form), because we do not know the form of the function from which it is derived. To integrate it we must have some transcendental function of higher order, which, in the course of time, may possibly become known.

But all transcendental functions do not require transcendentals of a higher order. Thus the integral of the trigonometrical functions, sin. $x\,dx$, tan. $x\,dx$, sec. $x\,dx$, &c., are expressed in other trigonometrical functions. Still it is more generally the case that integration raises a function to a higher transcendental, and the integral of this higher one demands still higher.

Thus $\int \frac{dx}{\sqrt{1-x^2}} = \sin.^{-1} x$; but $\sin.^{-1} x\,dx$ is not integrable.

$\int \frac{\cos. x\,dx}{\sin. x} = \log._e \sin. x$, but $\log._e \sin. x\,dx$ is not integrable.

$\int \frac{2\,dx}{x^3} = \frac{-1}{x^2}$, and $\int \frac{-dx}{x^2} = \frac{1}{x}$, and $\int \frac{dx}{x} = \log. x$, but log. $x\,dx$ is not integrable.

It is sometimes the case that algebraic expressions may be transformed so that their integral can be expressed in known transcedentals. Such is the case with the expression which forms the subject of this article. All differential expressions may be regarded as being derived from some function.

Hence, from this brief view of the subject, we infer, that if we knew all possible forms of transcendentals, we might integrate all differential expressions.

On a Mercurial Air Pump. By Mr. J Swan.

From the London Athenæum, Sept., 1863.

In general arrangement and appearance this instrument resembles a barometer, with very large lower reservoir, having an inlet and outlet pipe at the top of this, each provided with a stop-cock; and with the upper part of the barometer tube very greatly enlarged—in fact, a reservoir at the top, and a reservoir at the bottom. The upper reservoir, termed a vacuum-chamber, is surmounted by a ball-valve opening outward, and has also a tube with a stop-cock communicating with the vessel to be exhausted. The vacuum-chamber, tube, and a portion of the lower reservoir are, in the normal condition of the apparatus, to to be occupied by mercury. The remaining space within the lower reservoir is to be filled with water, which may be separated from the mercury by a caoutchouc bag, tied on the lower end of the tube containing the mercurial column. The inlet pipe entering the lower reservoir is to be connected with town water-pipes or a force pump. The working of the pump is effected by opening the outlet-pipe so as to permit the mercury to vacate the vacuum-chamber, and descend to the barometric level, displacing the water from the lower reservoir. Then the vacuum formed having been taken advantage of by opening the communication between the vacuum-chamber and the vessel to be exhausted, the original condition of things is restored by closing the outlet-pipe of the lower reservoir, and opening the inlet, so as to supply water at a high pressure, which will force the mercury to re-occupy the vacuum-chamber, the valve at the top allowing the exit of its more or less attenuated gaseous contents. This process, being frequently repeated, will, no doubt, give a very perfect vacuum, as there is no obstruction, of the nature of a valve, between the vacuum-chamber and the vessel to be exhausted. This air pump was said to be specially adapted for the exhaustion of small vessels. It was proposed that the instrument should be made entirely of wrought iron; among its advantages were small cost and simplicity, its efficiency not depending upon fine workmanship.

Proceedings British Association.

Locomotive Construction.

From the Lond. Mechanics' Magazine, Sept., 1863.

Notwithstanding the dictum contained in Clark's "Railway Machinery," we cannot regard the link motion as otherwise than imperfect. Its comparative excellence we admit; but with the growing tendency for higher pressures and early cut-off, its defects become day by day more prominent. In order to maintain anything like a full pressure on a piston, moving at 800 or 1000 feet per minute—a speed frequently reached in express engines—it is essential that all passages leading to the cylinder should not only be short, direct, and of large area, but that they should be opened and closed with a velocity proportionate to the speed of the piston. Locomotives are seldom worked in the central notches, except when running fast. The travel of the valve is then shortened, and the ports are not only reduced in area, but opened and closed with a tardiness, which greatly militates against the advantage to be derived from an early cut-off, diagrams taken at high speed showing a very remarkable loss of pressure as the piston pursues its course through the cylinder. Steam is estimated to flow into a vacuum with a velocity equal to that due to a body of the same density, falling through a space equal to the height of a column of steam of the given pressure. By this rule, steam of 120 lbs. pressure would flow into a vacuum at about 2079 feet per second; but the difference between the velocities of any two pressures, is the velocity with which steam would flow into steam of a lower pressure. At the commencement of a stroke, or rather at the moment when the valve first opens, the pressure within the cylinder is comparatively trifling, steam then enters with great velocity; once, however, the difference in the densities of the steam in the boiler and that in cylinders ceases to be well defined, steam passes very sluggishly from the former to the latter, and is little able to overcome the resistance offered by long curved passages and the contracted areas of the steam ports. It is very unusual, in consequence, to maintain more than 70 lbs. in the cylinder at high speed, indicator diagrams falling at once on the commencement of the stroke, long before the steam is really cut off. The analization of such diagrams is not easy, as it is impossible to tell, without setting out and measuring the valve gear, when the valve really closed. Theoretically, the ports should be extremely short, straight passages, opening directly from the valve-seat into the cylinder. The great size of the valve required to suit such an arrangement has hitherto precluded its adoption. Mr. Clark gives us good reason to believe that as much as 25-horse power is frequently expended in working valves as it is—a positive waste of power which it would be very injudicious to add to. If, however, the valves are properly balanced, there is nothing to prevent the employment of any size deemed most desirable; and if we are to adhere to the link and eccentrics, the sooner we have a large valve with a long throw, and great lap, the better. We illustrate this week a form of balance-valve, which, although recently patented here, has been worked for some time in America with considerable success. Plainmaison's slide-valves have been adopted on the Chemin de Fer du Nord since the year 1861 with excellent results both in the Crampton express and Engerth goods engines. Railway companies don't like change, however; and the general introduction of the balance-valve is, here at least, apparently as far off as ever.

Quitting the subject of valves, and resuming the consideration of the means by which they are put in motion, we find existing arrangements, lauded for simplicity which they do not really possess. The link motion, consisting as it does of two eccentrics with their rods, straps, pins, &c.; a link often built up of many pieces; and a rocking-shaft with balance weights or springs; is really the most complex part of the engine. Its expense is very considerable at first; and from the rapid wear to which it is exposed, and the nice adjustment which it must preserve, its maintenance forms a large item in the working expenses of the locomotive. We are well aware that this must be a distinguishing characteristic of any valve gear whatever; but we believe that other arrangements not a whit more complicated, will give better results. The mere fact of such a piece of machinery, delicate and finely wrought, answering its purpose so well, should rather encourage us in searching for a substitute which, not more simple, perhaps, would regulate the admission of steam to and its exit from the cylinders after a fashion approaching more nearly to theoretical excellence. Complexity is in itself no evidence of erroneous design. It only becomes reprehensible when it is introduced unnecessarily. In the construction ot machinery, the end usually sanctifies the means; and there is no reason that a valve gear may not be very excellent, and very complex as well. In America, the use of the separate cut-off was long adhered to, and is even still held in high favor by many builders. At least one locomotive in the great Exhibition last year was so fitted, and it seems likely enough that its general adoption is not far distant. The variation in the lead, when the reversing lever is in different notches, is not the least objection to the link motion. The setting of valves is in consequence a positive science, and one understood or at least practised, only recently. A few years ago it was quite exceptional to meet with a locomotive which "beat" equally. Even yet there is great room for improvement on most of our great railway lines.

The question of balance weights, at one time considered definitively settled, seems likely to be re-opened. It is extremely doubtful that it is good practice to perfectly balance the reciprocating parts of a locomotive. One locomotive superintendent has removed three-eighths of the accurate counterpoise from his engines, with manifest advantage. The solution of the problem lies, we fancy, in the fact that the pressure on the piston does not accurately represent that on the crank-pin, when the engine is in motion, because it is modified more or less by the momentum of the piston-rod, and connecting-rod—to such an extent, indeed, in some cases, that the strain on the crank-pin is much greater near the termination of a stroke than at its commencement, although the steam is early cut off. As the equalization of the impelling force on the crank-pin is extremely desirable, tending as it does to the steadiness of the entire machine, it is not improbable that a heavy cross-head, piston, and piston-rod may be better for high-speed locomotives cutting off very early, than those of a lighter construction. There can be no doubt, however, that the wheels must be accurately balanced, and this rule is seldom or never disputed. In conducting experiments on the subject, care must be taken to distinguish between the conditions under which an engine is tried in the shed, and those under which it performs its work on the road.

There is little tendency to alter the practice which deems a single pair of drivers sufficient for express engines. Locomotives intended for high speeds seldom or never are burdened with heavy trains, and with rails in good order they manage to proceed well enough, but, unfortunately, not without doing great mischief to the permanent way, in consequence of the excessive loads placed on the drivers to secure adhesion. Still there seems to be much weight in the arguments which are urged against coupling wheels over 7 feet in diameter, which make 270 revolutions betimes in the minute. The momentum of an 8-foot side-rod at such a pace would be something tremendous, and its fracture would lead to fearful results. For ordinary passenger trains there is no excuse for limiting the number of drivers to two, and coupled engines are rapidly getting into favor for this kind of work. For æsthetical as well as mechanical reasons, the second pair of driving-wheels should be placed behind the fire-box, not forward. If inside cylinders are adopted, the latter position throws all the work out of the horizontal line, and entails many difficulties with the smoke-box and framing. A few large engines of this class are doing good service on the North-Western, between Birmingham and Stafford. The driving-wheels are, we believe, 6 feet in diameter, and hence the cylinders do not come inconveniently near the ground.

Steel tyres make their way rapidly into favor with locomotive superintendents, while the civil engineer regards them with unmitigated disgust, from their destructive effort on permanent way. Six or seven tons on a single wheel was, however, more than any iron tyre, possessing toughness enough to be safe, could bear without spreading; and to the employment of hard irons which were resorted to as a remedy, may be attributed many of the accidents once so prevalent from the fracture of tyres. Railway companies have found it much to their advantage to purchase rails honestly made of good iron properly worked, instead of cheaper brands which proved utterly worthless. Steel tyres are not very injurious to such rails; still, a softer material is better for adhesion, cheaper to work, and perhaps safer, in some respects, than steel. If a tyre could be made, one with the wheel-rim proper, there is no doubt that its durability would be considerably increased; not only would its resistance to spreading be greater, but the shocks and vibrations to which it is exposed would be transmitted to the entire mass of the wheel, instead of being more or less retained in the tyre. Under the present system, the tyre is exposed to a treatment nearly similar to that which it received in the rolling mill which gave it existence, the interior being nearly as much injured by the wheel-rim, as the exterior is by the rail. Mr. G. S. Griggs, of the Boston and Providence Railroad, U. S., many years ago, set all his tyres on wood to avoid this action. The rims of the wheels are made with dovetailed recesses all round, running in the direction of the axle. Into these recesses hard wood-blocks, thoroughly dried, are firmly fixed, with the grain running in the direction of the groove. The tyres are then shrunk on, resting wholly on the wood, which stands, say, an eighth of an inch or less above the surface of the wheel-rim. Mr. B. Adams has brought out a far more elegant arrangement here. He introduces a continuous hoop spring, fitting within an internal groove in the tyre, between it and the wheel. Two of these hoops are, we believe, employed at present, each about one-third of an inch thick and 3½ inches wide. The wheel-rim is turned slightly convex, and rests on the hoops, which of course surround it. Experiments conducted on the St. Helen's Railway, Lancashire, show that Staffordshire tyres, fitted to wheels on this system, have given first-rate results when put in competition with Krupp's steel, Swedish, and Hood and Cooper's best iron, fitted in the common way. Krupp's tyres have run 40,873 miles, Hood and Cooper's 20,798, Swedish 34,006, without requiring turning up, as an average mileage; while Staffordshire tyres, fitted on springs, have run 55,138 miles, remaining in excellent condition. The engines had all 4 feet 6 inch wheels, except the last, which had 4 feet, and their weights varied from 19 tons 15 cwt., on Krupp's, to 26 tons

6 cwt. on Hood and Cooper's, 23 tons 14 cwt. on Swedish, and 21 tons on Mr. Adams' tyres. The saving to the rails must be considerable, for tyres can only be worn out at the expense of the rails. This same question of tyres nearly concerns the prosperity of railway companies. The maintenance of the permanent way is one of the heaviest items of expenditure which they have to encounter; and as the destroying element is found in the wheels of the locomotive, every improvement of which they are susceptible should be applied to them, without regard to the primary outlay, which is certain to repay itself a hundredfold.

Report by the Committee appointed to Investigate some Improvements in Gun Cotton.

From the London Athenæum, Sept., 1863.

The President said, the first business this morning was to receive an exceedingly important "Report by the Committee appointed to investigate some Improvements in Gun Cotton." It was a committee formed partly of members of the Mechanical Section and partly of members of the Chemical Section. The chemical part would chiefly occupy their attention.

Dr. Gladstone read the Chemical portion of the Report.—Since the invention of gun cotton by Prof. Schönbein, the thoughts of many have been directed to its application to warlike purposes. Many trials and experiments have been made, especially by the French; but such serious difficulties presented themselves that the idea seemed abandoned in every country but one, Austria. From time to time accounts reached England of its partial adoption in the Austrian service, though no explanation was afforded of the mode in which the difficulties had been overcome, or the extent to which the attempts had been successful. The committee, however, have been put in possession of the fullest information from two sources—Prof. Abel, chemist to the War Department, and Baron W. von Lenk, Major-General in the Austrian Artillery, the inventors of the system. Prof. Abel, by permission of the authorities, communicated to the Committee the information given by the Austrian Government to our Government, and also the results of his own elaborate experiments. General von Lenk, on the invitation of the Committee, by permission of the Austrian Government, paid a visit to this country, to give every information in his power on the subject, and brought over drawings and samples from the Imperial factory. The following is a summary of the most important points:—As to the chemical nature of the material, von Lenk's gun cotton differs from the gun cotton generally made, in its complete conversion into a uniform chemical compound. It is well known to chemists that, when cotton is treated with mixtures of strong nitric and sulphuric acids, compounds may be obtained varying considerably in composition, though they all contain elements of the nitric acid and are all explosive. The most complete combination (or product of substitution) is that described by M. Hadon as $C_{36}\ H_{21}\ (9\ NO_4)\ O_{30}$, which is identical with that termed by the Austrian chemists Trinitrocellulose, $C_{12}\ H_7\ (3\ NO_4)\ O_{10}$. This is of no use whatever for the making of collodion; but it is von Lenk's gun cotton, and he secures its production by several precautions, of which the most important are the cleansing and perfect desiccation of the cotton as a preliminary to its immersion in the acids—the employment of the strongest acids attainable in commerce—the steeping of the cotton in a fresh strong mixture of the acids after its first immersion and consequent imperfect conversion into gun cotton,—the continuance of this steeping for forty-eight hours. Equally necessary is the thorough purification of the gun cotton so produced from every trace of free acid. This is secured exclusively by its being washed in a stream of water for several weeks. These prolonged processes are absolutely necessary. It seems mainly from the want of these precautions that the French were not successful. From the evidence before the Committee it appears that this nitro compound, when thoroughly free from acid, is not liable to some of the objections which have been urged against that compound usually experimented upon as gun cotton. It seems to have a marked advantage in stability over all other forms of gun cotton that have been proposed. It has been kept unaltered for fifteen years; it does not become ignited till raised to a temperature of 136° C. (277° Fahr.); it is but slightly hygroscopic, and when exploded in a confined space, is almost entirely free from ash. There is one part of the process not yet alluded to, and the value of which is more open to doubt—the treatment of the gun cotton with a solution of silicate of potash commonly called water glass. Prof. Abel and the Austrian chemists think lightly of it; but von Lenk considers that the amount of silica set free on the cotton by the carbonic acid of the atmosphere is really of service in retarding the combustion. He adds, that some of the gun cotton made at the Imperial factory has not been silicated at all, and some imperfectly; but when the process has been thoroughly performed, he finds that the gun cotton has increased permanently about 3 per cent. in weight. Much apprehension has been felt about the effect of the gases produced by the explosion of gun cotton upon those exposed to its action. It has been stated that both nitrous fumes and prussic acid are among these gases, and that the one would corrode the gun and the other poison the artilleryman. Now, though it is true that from some kinds of gun cotton, or by some methods of decomposition, one or both of these gases may be produced, the results of the explosion of the Austrian gun cotton without access of air are found by Karolys to contain neither of them, but to consist of nitrogen, carbonic acid, carbonic oxide, water, and a little hydrogen and light carburetted hydrogen. These are comparatively innocuous; and it is distinctly in evidence that, practically, the gun is less injured by repeated charges of gun cotton than of gunpowder, and that the men in casemates suffer less from its fumes. It seems a disadvantage of this material as compared with gunpowder that it explodes at a temperature of 277° Fahr.; but against the greater liability to accidents from this course may be set the almost impossibility of explosion during the process of manufacture, since the gun cotton is always immersed in liquid, except in the final drying.[*] Again, if it should be considered advisable at any time, it may be stored in water, and only dried in small quantities as required for use. The fact that gun cotton is not injured by damp like gunpowder is, indeed, one of its recommendations, while a still more important chemical advantage which it possesses arises from its being perfectly resolved into gases on explosion; so that there is no smoke to obscure the sight of the soldier who is firing or to point out his position to the enemy, and no residuum left in the gun, to be got rid of before another charge can be introduced.

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[*] In ten years' experience it is proved that this temperature is sufficiently high to insure safety of manipulation; 277° Fahr. is an artificial temperature, and artificial temperatures accidentally produced are generally high enough to ignite gunpowder. The greater liability to accident from this cause can, therefore, scarcely be admitted.

Gun Cotton as an Explosive Agent.

From the London Practical Mechanic's Journal, October, 1863.

A Joint Committee of the British Association, emanating from two Sections, the Chemical and the Mechanical, was formed at the Cambridge Meeting, for the purpose of inquiring into the state of knowledge of gun cotton as an explosive agent. Wisely so; for while we in England have laid to for years, since the accident at the Dartford Powder Mills, early in the history of the discovery, continental nations have been steadily pursuing the subject, and with a very remarkable success; the Austrian Government having at this moment no less than *thirty-six batteries* of field artillery completely equipped for gun cotton ammunition.

Great interest was therefore excited at Newcastle at the late meeting of the British Association, by the presentation of the report of the above committee, which was read in both sections, but we cannot say discussed. The report on Baron Lenk's gun cotton made to the Austrian Government is a highly important document, and having through the kindness of Dr. Schneider procured a copy, we present it *in extenso* to our readers in English.

The report of the chemical side of the committee was also important, and a very long statement of experimental trials and technical details as to the best methods of manufacture on the Arsenal scale, and on the properties of gun cotton as a practical form of ammunition, the modes of preserving, &c., &c., read by Dr. Abel, the chemist to the War Department, were also highly important productions, which will appear in the forthcoming volume of reports of the Association. We cannot say quite as much for the mechanical side of the joint committee, read by Mr. John Scott Russell, for although comprising some interesting and curious discussion, it appeared to us to have been framed in a good deal of ignorance of what had been ascertained experimentally, and printed several years since by French officers and philosophers, and more especially by General Piobert and Colonel Mallet of the French Artillery on the subject of the Dynamic conditions of gun cotton explosion.

Baron Lenk has added in several respects to our prior knowledge, in the specialities of preparation of gun cotton, but his most decisive advance has been, his placing in a very clear light, if not his discovering the highly important fact, that the rapidity of the explosion of gun cotton, and with that, its *pouvoir brisánte*, may be modified to any extent by the amount of compression and the form of texture (so to say) given to it in the cartridge.

We shall return to the subject, and meanwhile may refer our readers for a *resumé* of Colonel Mallet's and General Piobert's investigations, and in general for a pretty clear view of the ballistics of the subject, to chapter 29, p. 127, *et seq.* of Mr. R. Mallet's work on Artillery.

REPORT ON BARON LENK'S GUN COTTON, BY PROFESSORS DR. REDTENBACHER, DR. SCHRÖTTER, AND DR. SCHNEIDER.

To His Excellency Field-Marshal Johann Freiherr Kempen von Fichtenstamm, President of the Royal Imperial Commission on Gun Cotton, June, 1863.

In accordance with your Excellency's wish, the undersigned submit the following opinions relative to the objections urged against the adoption of gun cotton for our purposes.

Introduction.—It is not possible within the limits of the present report to discuss all the various products which, since the discovery by Schonbein, in 1846, of a method of chemical treatment whereby cotton might be rendered explosive, have been brought forward, examined, and applied under the various names of "Gun Cotton," "Pyroxlyn," "Fulmi-coton," "Nitro cellulose," &c.,—or at any rate to do more than mention such as have been brought under our notice in the course of experiments performed by us with Baron Lenk's gun cotton.

It is true that gun cotton has frequently and by various persons been manufactured by modifications of one of the two following processes, viz:—

(1) By acting on cotton with mixtures of nitric and sulphuric acid.
(2) By acting on cotton with mixtures of saltpetre and sulphuric acid.

These two general processes have been so much varied in detail by different experiments, that the resulting specimens of gun cotton must necessarily have been most diverse in quality. The variations of detail may be classified as follows, viz:—

(1) Variations as to strength of acids.
(2) Variations as to the conditions of mingling the acids.
(3) Variations as to duration of chemical action.
(4) Variations as to temperature.
(5) Variations as to the removal of free acid from the resulting gun cotton.

These variable conditions being taken into consideration, it is no longer a matter of surprise that even the chemical composition of gun cotton has been differently stated by different authorities. Accordingly whilst one set of chemists, in explaining the *rationale* of action whereby ordinary cotton is changed into gun cotton, refer the change to a substitution of one equivalent of hyponitric acid for three equivalents of hydrogen in the original cotton; another set affirm that four equivalents represent the hydrogen exchanged; and yet other chemists, five equivalents. In addition to these three definite compounds, various analyses have testified to an infinity of secondary bodies, the result of mechanical mixtures.

Unforeseen explosions of gun cotton occurring without any known cause, and assumed to have been the result either of spontaneous combustion, or of some unimportant elevation of temperature, as up to 50° or 60° C., have frequently been chronicled in the short history of this substance; whilst on the other hand, reliable authorities have fixed the exploding temperature between 130° and 160° C.,—another proof as to the variety of materials operated upon on a small scale. The chief experiments relative to gun cotton have been made by chemists upon small portions, one pound being looked upon as a considerable quantity. Results based on such data may be considered on a par with general conclusions as to the properties of gunpowder founded upon experiments with a mixture by hand of charcoal, sulphur, and nitre, in quantities yielding a pound weight aggregate.

Chemical changes involved in the manufacture of gun cotton.—§ 2. The fact should be borne in mind, that the changes which cotton undergoes in its treatment with nitric and sulphuric acids conjointly, are not alone peculiar to cotton and other vegetable fibre, but that a great number of organic bodies are amenable to a corresponding change if they be similarly treated. The exact grade of mutation which any particular organic substance may undergo when treated as above will differ, however, according to circumstances. Even if, as a rule, we assume that for every equivalent of water displaced, an equivalent of nitric acid is appropriated—or, what amounts to the same thing, if the assumption be that every single equivalent of hyponitric acid exchanges with a corresponding equivalent of hydrogen—still variations of result may be accounted for by reference to the modifying conditions already indicated. However, the general deductions may be arrived at, that the amount of hyponitric acid appropriated is directly as the amount of hydrogen lost.

Possibly, the results obtained by chemists on the small scale, using small amounts of cotton and relatively large amounts of acid, may not be identical in composition with corresponding results of large scale operations; time in both cases being constant. It may also be fairly assumed that certain portions of the original cotton may escape nitrification, wholly or partially. These suggestions tend to show the little value that can justly be attached to past experience with gun cotton, if cited in reference to the specific gun cotton of Baron Lenk.

Experiments by the French.—§ 3. An extensive series of experiments with gun cotton must, however, not be passed over; the material having been produced commercially on a large scale, and the investigation moreover conducted by military and scientific authorities. A record of these experiments may be seen in Berthier's "Mémoire de l'Artillerie," Paris, Bathelier, 1852.

Immediately after the discovery of gun cotton in 1846, the French Minister appointed a commission of inquiry relative to it.

For a period of six years this commission prosecuted its investigations, and the results are printed in the pamphlet above mentioned.

These experiments were conducted on a large scale; five tons of gun cotton being manufactured. This commission instituted the most searching inquiries relative to every possible ballistic and other military use to which gun cotton could be applied. No less than thirty tabulated digests resulted from the labors of this French committee.

Experiments so seemingly exhaustive might reasonably prompt the assumption that no point relative to gun cotton and its application had remained undetermined. It might seem that the subject of gun cotton was definitely set aside after the decisive verdict which the following quotation records:—

"Dans l'état actuel, il n'y a pas lieu de continuer les expériences au point de vue de leur emploi dans les armes de guerre."

Perusal of the report leaves no doubt either as to the pertinence of questions raised, or the intelligence manifested in the corresponding replies.

The French neglected to ascertain the true composition of the cotton.—One point, however, was lost sight of by the French commission, namely the precise composition of the gun cotton operated upon. On this point the commissioners are silent: hence the question, *whether one and the same chemical compound formed the subject of every experiment in the series*, was never raised. The report throughout is based on the assumption that identical methods of treatment must yield identical results; that no analysis was required; that practice in firing supplied the *ultima ratio;* and lastly, that the results of explosion were referable to chemical composition wholly and absolutely. The first proposition is usually, but not *invariably* true, as every chemist knows.

Minor circumstances frequently influence the result of chemical energy, though primary conditions remain unchanged. Chemists sometimes, heeding these primary conditions only, imagine they have continued to do the same thing and to achieve the same result; whereas different acts have been performed, and different results accomplished. This has happened in prosecuting the manufacture of gun cotton, up to the experiments of Baron Lenk. What *may* take place during the change of ordinary cotton into gun cotton, under different conditions, can be thus expressed. The cellulose (matter of cotton) may yield up successive increments of hydrogen, to be replaced by hyponitric acid, according as the acid bath employed varies as to relative bulk, and the time of action varies. One equivalent of hydrogen is removable more easily than two, two than three, &c., &c.

In this way, not only may specimens of gun cotton (as commonly prepared) be made up of variable mechanical mixtures, of every chemical result possible under the circumstances, but the series may be still further extended by the interpolation of material upon which no chemical action has been exerted. Hence may be deduced a most important distinction between the French gun cotton and that of Baron Lenk.

Difference between the French gun cotton and Baron Lenk's.—According to the method pursued by the French Commission, the raw cotton was immersed in the acid mixture for one hour.

Baron Lenk leaves his cotton forty-eight hours in the acid bath.

The French cotton was afterwards dipped in running water for an hour or an hour and a half.

Baron Lenk's gun cotton lies four, six, or eight weeks in a stream.

The French cotton had after washing, so much free acid left, that wood-ash ley (a solution of carbonate of potash, therefore,) was neutralized by contact with it, and after long use, became sour.

Baron Lenk's cotton is so freed from acid by long immersion, that a two per cent. solution of potash, in which two cwt. of gun cotton had been boiled, had lost none of its alkaline properties; that is to say, that the cotton was completely free from acids, as experiments wholly accordant with those of the Imperial (Austrian) Engineers' Committee fully demonstrated. The French gun cotton having been prepared in a manner so different, it must necessarily have had a different composition to that of Baron Lenk's; hence it is clear that the French experimental results cannot, without considerable reserve, be accepted as precedents.

Lenk's cotton is Tri-nitro Cellulose and uniformly composed.—It manifested no inconsiderable amount of confidence on the part of Baron Lenk, when the French experiments were known to him, that he did not give up the gun cotton altogether; and he can only have been supported by the conviction based on a thoroughly grounded study of the entire process.

Baron Lenk calls the Hirtenberg cotton (and with full right) his cotton, as he prepares it by one constant, definite, and unvarying process.

Baron Lenk takes the strongest preparation of acid, one part nitric acid to three parts sulphuric acid: he impregnates a spun thread of cotton in a boiling solution of potash thoroughly, and leaves the acids to act cold upon the cotton during forty-eight hours.

These details of manufacture are all important; and the resulting cotton is also most convenient for the subsequent process of freeing from acid, drying, and working up; whilst during the time employed, the three most easily removed hydrogen equivalents of the cellulose are completely replaced by hyponitric acid, and during this time the action takes place through the body of the cotton uniformly, so that no isolated portions are less azotized than others; nor if properly conducted, is any portion too highly azotized; in fact, it can be demonstrated that the Austrian gun cotton contains three easily-removed equivalents of hydrogen.

7. *Analysis.*—This is demonstrated by the recorded analysis of the Austrian Imperial Engineers' Committee, 1861 (vol. i. part 1, p. 21), wherein it is shown that Lenk gun cotton is almost wholly composed of tri-nitro cellulose.

ANALYSIS OF AUSTRIAN GUN COTTON.

Laboratory of Engineers' Committee, 1861.

In 100 parts,	Tri-nitro Cellulose calculated.	No. 4.	Original Cotton.
Carbon,	24·3	25·1	
Hydrogen,	2·3	3.0	

University Laboratory, 1863.

No. 3. 1856.		No. 6. 1860.		No. 14. 1862.			Di-nitro Cellulose.
1	2	1	2	1	2	3	
24·4	24·5	24·6	24·2	23·6	23·9	24·1	28·6
2·7	2·8	2·6	2·7	2·6	2·4	2·4	3·2

If this analysis differs somewhat from the theoretical formula of the tri-nitro cellulose, the circumstance must be remembered that cotton is not pure cellulose, but that it consists of long-extended vegetable cellules, in which there is always a little albuminous substance containing over 50 per cent. carbon, and 7 per cent. hydrogen, the presence of which even in such quantities easily increases the per centage of carbon and hydrogen.

The treatment with soluble glass has no influence on Baron Lenk's gun cotton, it being previously free from acids.

Gun Cotton is always put into comparison as an explosive compound with gunpowder; but it must be remembered that one of the component parts of gunpowder—charcoal—is most irregular in quality, especially where the primitive method of preparing it is followed. Still in theoretical disquisitions upon gunpowder, charcoal is taken into account as pure carbon.

Bunsen has so treated the charcoal in his experiments with Berne powder and Austrian army powder at the University Laboratory.

	Berne.	Austrian.	Powder.
Carbon, . .	68·84	81·2	82·9
Hydrogen, . .	3·67	2·8	2·09
Oxygen, . .	27·49	13·6	12·1
Ashes, . .	——	2·4	2.

It is, however, well known that the Austrian charcoal is manufactured at a higher temperature.

The French chemist Violette, who effected the latest improvements of charcoal manufacture, proved a difference in charcoal of eight per cent. in hydrogen, and one per cent. ashes; and the comparative proportions of charcoal of the same wood made at various temperatures varied from 340° to 800° C.

If it be considered that with us the manufacture of gunpowder as well as of charcoal is in the hands of ignorant workmen of the lowest class, who make at one operation, and of the same wood, charcoal at all sorts of temperatures, then the opponents of gun cotton, who imagine they have in powder the perfection of shooting material, are of all persons the least justified in the reproach that Baron Lenk's gun cotton is not uniform in its composition.

This want of uniformity in the composition of powder-charcoal was the origin of the tests by the Berthier process for discovering, before mixing, the component parts of powder, especially the powder-charcoal, by means of the Berthier process or trial, for the purpose of ascertaining its combustive value.

Unalterable quality of the Lenk Gun Cotton.—Even if invariability of composition in respect of gun cotton be conceded, the allegation is made, that if kept for a long time it deteriorates; the ground of this dictum being sought in experiments which show the alteration of the gun cotton to the extent of 100° with regard to litmus paper, and also are assumed to reveal a spontaneously explosive quality. The changes in the Hirtenberg gun cotton can only be ascertained direct by comparison and analysis with other and different samples of gun cotton made on the same day when manufactured, and then by repetition of analysis of the same portions of cotton after one, two, three, or more years; these results finally compared with the first year's analyses: but there exist no such analyses, because they were never made. The opponents of gun cotton themselves have therefore omitted the only true proof of a changeable quality. Such omissions can, nevertheless, be in some degree remedied.

In the magazines of gun cotton at the Neustadter Haide, there are stores of various years.

In the laboratory of the University there are samples of Hirtenberg gun cotton of three several years, which have been examined by the above-named artillery officers, and they have been found not to differ materially in their composition from tri-nitro-cellulose. For instance—

Per Centage.	Tri-Nitro Cellulose.	No. 3 of the year 1856.		No. 6, year 1860.		No. 14, year 1862.		1863.
		1	2	1	2	1	2	3
Carbon, .	24·3	24·1	24·5	24·6	24·2	23·6	23·9	24·1
Hydrogen, .	2·3	2·7	2·8	2·6	2·7	2·6	2·4	2·4

If these results be compared with each other, there can be no right to say that Hirtenberg gun cotton alters by keeping. They agree so far with each other as analyses of the *same material* usually do. It is to be regretted, on this as on many other accounts, that during the last twelve years such analyses were not frequently repeated. If the opponents of gun cotton, in performing an adverse experiment, heat the substance in a test tube up to 100° C., and holding litmus paper over it, deduce from redness of the latter that gun cotton changes after long keeping, they merely prove thereby that gun cotton changes at 100° C. Of an explosive compound, it can only be required that *it shall not deteriorate within certain limits of temperature; a requisition amply fulfilled by Lenk's gun cotton.*

Some varieties of gun cotton, if enclosed together with litmus paper in a tube, often manifest an acid reaction at ordinary temperature. This may arise from various causes. There may exist, for example, free acids. These acids may be the result of nitrogen partly oxidized. and may result from imperfectly worked cotton. This assumption granted, the phenomenon is explained, and the cause is easily avoided.

It may arise from decomposition of the gun cotton, atmospheric dampness having brought about a partial reconstitution of the cellulose.

This assumption granted, the acid reaction just adverted to should be local, affecting the material in particular spots only. This seems a necessary deduction, having regard to the long process of steeping in water to which Lenk's gun cotton is exposed in the course of manufacture. By this steeping process the cotton should otherwise be utterly destroyed.

Possibly the tetra and penta nitro cellulose are more liable to decomposition; but only traces of these bodies are to be found in Lenk's gun cotton, as experiment amply demonstrates. Were it otherwise, analyses should make known (which they do not) a deficiency of carbon and hydrogen below the theoretical quantity.

		Carbon.	Hydrogen.
Tri-nitro, . .	Cellulose.	24·2	2·3
Tetra-nitro, . .		21·0	1·7
Penta-nitro, . .		18·6	1·3

But some specimens of Lenk's cotton do not even yield traces of decomposition. A parcel of Hirtenberg cotton was laid for six weeks in a pond, and not subsequently treated with potash. It was then deposited in a running stream, afterwards exposed for one month to the air; being subjected to all the various influences of dew, rain, and sun, day and night continuously. It retains all its original explosive qualities, and fails to redden litmus paper, even though the latter be wrapped in a mass of this cotton and allowed to remain for many days. The results of an analysis of this cotton were almost identical with the calculated elements of tri-nitro cellulose, as the following table makes apparent:—

	Calculated.	Found.
Carbon, . . .	24·2	24·4
Hydrogen, . . .	2·3	2·8

The redness of litmus paper that sometimes occurs may also result from traces of organic acids, formic and acetic acids for example, both which may emanate from the turpentine of the pine chests in which the gun cotton is stored.

These acid traces should the less evoke surprise, when we bear in mind that the gun cotton in process of manufacture had been exposed for forty-eight hours to a strong acid bath; moreover, if the subject of comparison, viz: gunpowder, be tested with equal severity, similar evidence of chemical action would be forthcoming. The courses of testing must, however, be different.

Spontaneous Decomposition of Gunpowder.—Gunpowder contains sulphur as is well known. Sulphur being an element cannot be decomposed; nevertheless, it is affected by the atmosphere, though slowly.

The change here is oxidation. the product being sulphurous acid. This oxidation imperceptibly affects solid cakes or bars of sulphur, but more rapidly if the sulphur be powdered. Now gunpowder contains sulphur in the highest degree of comminution, and if gunpowder be smelt, sulphurous acid is easily perceived. Other evidence is, however, available.

The sulphurous acid resulting from decomposition of sulphur is slowly changed into sulphuric acid, which latter partly attacks the saltpetre, and is partly neutralized by the potash of the ashy parts of the charcoal.

Gunpowder has a weak alkaline action, although each of the constituents of gunpowder is absolutely neutral. The alkilinity can only result from potash contained as an impurity in the charcoal. These facts borne in mind, it follows that any sulphuric acid (resulting from sulphurous acid), developed in gunpowder, should be found united with potash or sulphate of potash. Now the presence of sulphuric acid is easily demonstrable in affected gunpowder by the test of chloride of barium; and inasmuch as sulphuric acid does not exist in either of the original constituents, it must necessarily be a product of decomposition.

These observations are only intended to show that no great importance should be attached to the casual reddening of litmus paper by gun cotton. Parity of reasoning should lead to the rejection of gunpowder; and, indeed, gunpowder by long exposure to atmospheric influences is so deteriorated as to be rendered wholly unfit for ballistic uses.

Temperature at which Gun Cotton Ignites.—The rejection of gun cotton, in consequence of the changeable nature of, or the explosive quality of the material at low temperatures of the material, is so thoroughly and decidedly contradicted in the Report of Baron von Ebner, that it would be superfluous to go any further into this question; the lowest explosive temperature of the Hirtenberg gun cotton being therein fixed at 136°; a temperature which, practically, cannot raise any doubts against the use of gun cotton.

(To be Continued.)

For the Journal of the Franklin Institute.

On the Parabolic Construction of Ships. By John W. Nystrom, C. E.

(Continued from vol. xlvi, page 396.)

It has not yet been explained how a ship is constructed by the parabolic method without calculation. Shipbuilders hesitate to study the system on account of the numerous algebraical formulas connected with it, and with the common prejudice pronounce it "theoretical." The very few shipbuilders on whom I have succeeded in impressing the importance of the parabolic method, are well convinced and have acknowledged that I employ less calculation than they have done for the same object; the formulas are numerous, because the method has revealed a great many important rules in shipbuilding, which were never before thought of, and each rule is brought to the simplest possible form.

In the practical application of the parabolic method we may proceed to construct a ship without calculation, as follows:

Having given the principal dimensions of a vessel, say 250 feet long in the water-line, beam 30 feet, and 12 feet draft of water, construct a diagram like Plate I, divided into 100 parts each way, the side B C to be a little longer than half the beam of the vessel, at whatever scale it is to be constructed. Set off half the beam from B towards C, which may be B *b*; join *b* with A. Select from Plate II, (vol. xlvi, p. 396.) a desired sharpness of the vessel, and we may adopt the figures marked 2 and 2·75 (2·75 is a sharpness between the figures 2½ and 3), which means that the exponent will be 2 for the forward and 2·75 for the aft load water-line. Draw the centre line, and set off the length of the vessel, locate the dead-flat 𝔛 according to your own judgment, and divide the fore and aft parts each into 8 equal parts, numbered as shown on Plate II. In the accompanying Table III, find the selected exponent 2 in the first column, which line contains the ordinates for the forward water-line. The fourth ordinate is 7214; find this number on the base A B, Plate I, which is at *a*, and the height *a c* is the length of the fourth ordinate for the forward water-line. The second ordinate is 3732 in the table, which will be at *d* on the diagram, and *d e* the length of the second ordinate. The remaining ordinates are found in the same way; lay them out on the plan, and draw the water-line. For the aft water-line, the ordinates in Table III are in the line of the assumed exponent 2·75. Draw the centre line for the frame drawing or body plan, set off and divide the depth, 12 feet, into 8 water-lines, as shown on Plate II, last volume; number them from the keel to the load water-line. Select on Plate II a desired shape of the dead-flat cross-section 𝔛, which may be assumed to the figure marked 4, and means that the exponent is 4. In Table II, exponent 4, the fourth ordinate is 9375. which is at f on the diagram, and the line $f\,g$ the length of the fourth ordinate. Find in the same way the remaining ordinates, lay them out on the body plan, and draw the frame; continue it above the water-line, according to your taste and the purposes of the vessel. Draw the side view of the ship, form the stem and stern as you please, project the extremities of the rail from the side view to the plan. Set off the rail-beam from the frame drawing to the plan, and locate its dead-flat as you please, say a little forward of the dead-flat of the water-line. Select the exponents for the rail, say 3·75, Table II, for the forward rail, and 2·75, Table V, for an elliptic stern. Divide the parts from the dead-flat to the extremities of the rail each into 8 equal parts, and number them towards the centre; set off from B on the diagram half the rail-beam, which we may assume to be B 7; find the ordinates in the tables, and set them off from A B to A 7, as described for the water line and frame $\mathfrak{X}$.

Now we are obliged to perform a calculation to find the area of half the immersed cross-section of the dead-flat frame, which is obtained by multiplying together half the beam, 15 feet, and the draft of water, 12 feet, and the product by the area co-efficient for the exponent 4, Table II, which is 0·8, or

$$\tfrac{1}{2}\,\mathfrak{X} = 12 \times 15 \times 0{\cdot}8 = 144 \text{ sq. feet.}$$

Select the exponent for the displacement somewhere near that of the forward water-line, say 2·375, Table IV. The co-efficient for the fourth ordinate cross-section is ·6514, and the area

$$\omega = 144 \times 0{\cdot}6514 = 93{\cdot}8 \text{ sq. feet.}$$

Now it is required to construct the shape of the fourth frame. The co-efficient for the fourth-line ordinate was 0·7214, which multiplied by half the beam, 15 feet, and the product by the draft, 12 feet, gives the area of the rectangle = 129·85, from which subtract 93·8 sq. feet, and the balance, 36·05, divided in 93·8, gives the exponent for the fourth frame, 2·438. Had this exponent fallen in with any one in Table II, we would have had the ordinates for the fourth frame directly from that table; but we find it to be just half-way between 2·375 and 2·5, for which we must refer to the diagram, Plate III, last volume, where we find the co-efficients for the ordinates over the exponent, 2·438, to be 4th = 812.

Set off from B on the accompanying diagrams the seven forward water-line ordinates, number and join them with A, as shown; set off from D the aft water-line ordinates, and you have a scale for all the ordinates in the sixteen frames. The fourth ordinate, 812, at h on A B, gives the length, $h\,i$. The second ordinate over the exponent 2·438, Plate II, is 275, which at k gives the length $k\,l$, and so all the frames are constructed as you see, without reference to water-lines or diagonals.

Should the shape of the frames thus formed not agree with your taste, alter them as you please, only be careful that whatever is taken in or out in one place must be taken out or in in another, so that the area will remain the same as calculated.

TABLE II.

Parabolic Construction of Ships.

Expo.	Ordinates for Frames. $b = 1$.							Area.	Cent Gr.	Index.
n'	1	2	3	4	5	6	7	$\frac{M}{B\,l} =$	$e - d$	i.
0·125	·0166	·0353	·0571	·0830	·1154	·1591	·2289	·1111	·2647	1·276
0·25	·0328	·0693	·1109	·1591	·2179	·2929	·4054	·2000	·2777	1·468
0·375	·0488	·1023	·1616	·2289	·3081	·3925	·5415	·2727	·2819	1·641
0·5	·0646	·1349	·2094	·2929	·3876	·5000	·6464	·3333	·3000	1·771
0·625	·0800	·1646	·2545	·3514	·4583	·5796	·7274	·3846	·3095	1·882
0·75	·0949	·1941	·2971	·4046	·5208	·6464	7898	4286	·3182	1·918
0·875	·1098	·2225	·3372	·4539	·5761	·7027	·8379	·4666	·3261	1·981
1	·1250	·2500	·3759	·5000	·6250	·7500	·8750	·5000	·3333	2·000
1·125	·1398	·2765	·4107	·5415	·6683	·7898	·9013	·5294	·3400	1·988
1·25	·1537	·3020	·4443	·5771	·7066	·8232	·9256	·5555	·3461	1·969
1·375	·1692	·3267	·4760	6145	·7404	·8514	·9386	·5789	·3518	1·929
1·5	.1815	·3505	·5059	·6465	·7703	·8750	·9558	·6000	·3571	1·897
1·625	·1959	·3734	·5341	·6753	·7969	·8949	·9659	·6190	·3621	1·848
1·75	·2081	·3956	·5607	·7023	·8203	·9116	·9737	·6363	·3666	1·833
1·875	·2211	·4169	·5857	·7269	·8421	·9256	·9797	·6522	·3710	1·801
2	2375	·4376	6094	·7500	·8593	·9376	·9844	·6666	·3750	1·771
2·125	2470	·4574	·6317	·7707	·8756	·9474	·9879	·6800	·3788	1·743
2·25	·2595	·4766	·6527	·7897	·8899	·9558	·9907	·6923	·3823	1·716
2·375	·2716	·4950	·6725	·8071	·9026	·6628	·9928	·7040	·3857	1·692
2·5	·2838	·5129	·6912	·8232	·9139	·9687	·9944	·7142	·3888	1·667
2·625	·2954	·5191	·7088	·8377	·9238	·9737	·9957	·7241	·3919	1·646
2·75	·3073	·5466	·7254	·8513	·9326	·9779	·9967	·7333	·3948	1·624
2·875	·3185	·5627	·7411	·8634	·9404	·9814	·9974	·7419	·3974	1·595
3	·3301	·5781	·7558	·8750	·9472	·9844	·9980	·7500	·4000	1·587
3·125	·3413	.5932	·7699	·8856	·9536	·9872	·9984	·7575	·4024	1·569
3·25	·3521	·6074	·7829	·8944	·9587	·9889	·9988	·7647	·4047	1·551
3·375	·3629	·6218	·7957	·9036	·9633	·9907	·9991	·7714	·4069	1·537
3·5	·3733	·6346	·8078	·9116	·9677	·9922	9993	·7777	·4091	1·520
3·625	·3839	·6478	·8185	·9192	·9719	·9935	·9994	·7838	·9111	1·566
3·75	·3939	·6600	·8284	·9256	9747	·9944	·9995	·7894	·4130	1·492
3·875	·4040	·6722	·8383	·9320	·9780	·9956	·9996	·7948	·4150	1·480
4	·4138	·6836	·8474	·9375	·9802	·9961	·9997	·8000	·4166	1·468
4·25	·4332	·7065	·8654	·9481	·9848	9971	·9997	·8095	·4200	1·435
4·5	·4517	·7260	·8794	.9557	·9878	·9978	·9998	·8181	·4230	1·424
4·75	·4700	·7455	·8934	·9633	·9968	·9985	·9998	·8260	·4259	1·406
5	·4871	·7627	·9046	.9687	·9926	·9990	·9999	·8333	·4286	1·389
5·25	·5042	·7798	·9158	·9741	·9943	·9992	·9999	·8400	·4301	1·372
5·5	·5202	·7945	·9246	·9779	·9954	·9994	·9999	·8461	·4333	1·358
5·75	5362	·8092	·9384	·9817	·9965	·9996	·9999	·8533	·4355	1·349
6	·5512	·8220	·9404	·9843	·9972	·9997	1·000	·8571	·4375	1·332
6·5	·5802	·8459	·9528	·9889	·9983	·9998	1·000	·8666	·4412	1·309
7	·6073	·8665	·9627	·9928	·9989	·9998	1·000	·8750	·4444	1·289
7·5	·6336	·8845	·9713	·9945	·9994	·9999	1·000	·8823	·4473	1·275
8	6564	·8989	9767	·9960	·9996	·9999	1·000	·8888	·4500	1·256
9	·6993	·9249	·9854	·9985	·9998	·9999	1·000	·9000	·4545	1·230
10	·7369	·9437	·9909	·9990	·9999	1·000	1·000	·9090	·4583	1·208
12	·7963	·9683	·9976	·9998	1·000	1·000	1·000	·9231	·4643	1·175
16	·8819	·9991	·9999	1·000	1·000	1·000	1·000	·9412	.4720	1·133

TABLE III.

PARABOLIC CONSTRUCTION OF SHIPS.

EXPO.	ORDINATES FOR WATER-LINES. $b = 1$.							AREA.	$s =$	RESIST.
n.	1	2	3	4	5	6	7	$\mathfrak{a} =$ BL×	$l' - l$.	k.
2	·1697	·3732	·5647	·7214	·8433	·9303	·9825	·6491	·0527	1·95
2·125	·1879	·3904	·5865	·7456	·8610	·9416	·9866	·6592	·0531	1·97
2·25	·1944	·4074	·6068	·7620	·8754	·9500	·9895	·6730	·0535	1·98
2·375	·1952	·4244	·6271	·7834	·8898	·9584	·9924	·6817	·0533	1·99
2·5	·1960	·4415	·6460	·7973	·9013	·9642	·9943	·6939	·0532	2·00
2·625	·2046	·4582	·6446	·8138	·9127	·9701	·9954	·7035	·0528	1·99
2·75	·2127	·4744	·6817	·8277	·9219	·9744	·9962	·7124	·0523	1·98
2·875	·2207	·4905	·6985	·8417	·9311	·9785	·9970	·7210	·0517	1·96
3	·2284	·5063	·7143	·8537	·9383	·9817	·9977	·7287	·0510	1·94
3·25	·2445	·5368	·7439	·8760	·9513	·9870	·9986	·7433	·0496	1·91
3·5	·2596	·5657	·7706	·8895	·9617	·9907	·9992	·7654	·0482	1·88
3·75	·2779	·5947	·7954	·9114	·9699	·9924	·9995	·7682	·0463	1·85
4	·2962	·6193	·8164	·9247	·9762	·9953	·9997	·7789	·0452	1·82
4·5	·3337	·6670	·8534	·9463	·9853	·9976	·9998	·7975	·0424	1·76
5	·3744	·7105	·8839	·9619	·9909	·9988	·9998	·8139	·0390	1·70
5·5	·4081	·7465	·9070	·9727	·9944	·9994	·9999	·8269	·0375	1·64
6	·4428	·7790	·9260	·9806	·9965	·9997	·9999	·8385	·0354	1·58
6·5	·4703	·8266	·9405	·9860	·9978	·9998	1·000	·8479	·0352	1·52
7	·5081	·8328	·9533	·9902	·9988	·9999	1·000	·8582	·0327	1·46
8	·5861	·8794	·9719	·9953	·9994	1·000	1·000	·8730	·0286	1·34
9	·6186	·9047	·9815	·9975	·9998	1·000	1·000	·8852	·0261	1·28
10	·6606	·9274	·9883	·9992	·9999	1·000	1·000	·8942	·0237	1·18
12	·7415	·9601	·9954	·9997	1·000	1·000	1·000	·9110	·0206	1·08
16	·8481	·9871	·9993	·9999	1·000	1·000	1·000	·9319	·0156	1·00

TABLE IV.

DISPLACEMENT OF VESSELS.

EXPO.	ORDINATE CROSS-SECTIONS $\mathfrak{O}$. $\mathfrak{M} = 1$.							DISPLACEMENT.		CENT. GR.
n''.	1	2	3	4	5	6	7	r = $\mathfrak{M}$ L×	T = $\mathfrak{M}$ L×	$s = l\times$
2	·0545	·1914	·3713	·5625	·7384	·8909	·9698	·5333	·0152	·3125
2·125	·0610	·2092	·3991	5940	·7667	·8976	·9759	·5506	·0157	·3160
2·25	·0673	2271	·4260	·6236	·7919	·9135	·9815	·5663	·0162	·3189
2·375	·0737	·2450	·4523	·6514	·8147	·9270	·9857	·5813	·0166	·3278
2·5	·0805	·2512	·4777	·6777	·8352	·9383	·9889	·5952	·0170	·3333
2·625	·0872	·2695	·5024	·7017	·8534	·9481	·9914	·6083	·0173	·3388
2·75	·0944	·2987	·5262	·7247	·8697	·9563	·9937	·6204	·0177	·3412
2·875	·1014	·3166	·5492	·7455	·8844	·9631	·9948	·6325	·0181	·3469
3	·1090	·3342	·5713	·7667	·8972	·9691	·9960	·6429	·0184	·3500
3·25	·1239	·3689	·6129	·7999	·9191	·9779	·9976	·6629	·0189	·3571
3·5	·1394	·4027	·6512	·8310	·9365	·9845	·9986	·6806	·0194	·3636
3·75	·1551	·4356	·6862	·8567	·9500	·9888	·9990	·6968	·0199	·3697
4	·1712	·4673	·7181	·8789	·9608	·9922	·9994	·7111	·0203	·3750
4·5	·2039	·5270	·7736	·9146	·9751	·9956	·9996	·7364	·0211	·3846
5	.2373	·5817	·8183	·9384	·9853	·9980	·9998	·7575	·0216	·3929
5·5	·2706	·6312	·8548	·9563	·9908	·9988	·9998	·7755	·0221	·4000
6	·3038	·6757	·8844	·9688	·9944	·9995	·9999	·7924	·0227	·4062
6·5	·3366	·7155	·9078	·9780	·9966	·9996	·9999	·8047	·0230	·4108
7	·3688	·7508	·9268	·9845	·9978	·9997	·9999	·8170	·0233	·4166
8	·4309	·8080	·9540	·9920	·9992	·9998	1·000	·8366	·0239	·4250
9	·4890	·8554	·9710	·9970	·9996	·9998	1·000	·8521	·0244	·4318
10	·5430	·8906	·9819	·9980	·9998	·9999	1·000	·8656	·0248	·4375
12	·6341	·9375	·9934	·9996	·9999	1·000	1·000	·8861	·0253	·4464
16	·7777	·9991	·9998	·9999	1·000	1·000	1·000	·9126	·0261	·4629

TABLE V.

For Elliptic Stern of Vessels.

Expo.	Ordinates of Ellipses of Different Order.								Area.	Index
n.	$\frac{1}{2}$	1	2	3	4	5	6	7	$\mathbf{a} = bl \times$	i.
2	·3398	·4840	·6616	·7808	·8660	·9204	·9682	·9922	·7854	1·603
2·25	·4108	·5490	·7147	·8274	·9004	·9495	·9801	·9958	·8154	1·544
2·5	·4670	·6042	·7657	·8627	·9252	·9546	·9873	·9978	·8382	1·495
2·75	·5174	·6514	·8029	·8901	·9434	·9749	·9932	·9989	·8564	1·453
3	·5604	·6911	·8331	·9019	·9565	·9821	·9948	·9994	·8709	1·418
3·25	·5991	·7252	·8578	·9275	·9664	·9871	·9973	·9996	·8833	1·388
3·5	·6333	·7548	·8782	·9406	·9740	·9907	·9978	·9998	·8935	1·362
4	·6096	·8021	·9003	·9595	·9840	·9950	·9995	·9999	·9075	1·318
Arc.	30°	·0149	·0582	·1321	·2374	·3740	·5449	·7531	Sheer of Vessels.	
	45°	·0157	·0539	·1221	·2152	·3517	·5227	·7313		
	60°	·0160	·0474	·1086	·1972	·3190	·4794	·6946		

When the form of the vessel is thus constructed, divide the frames as required in building it, and finish the drawing accordingly.

It is not considered necessary to describe how to continue the frames to the rail, which the ship's constructor can do in the ordinary way. The sheer is set off from B to S, and the ordinates taken from Table V.

The displacement of the vessel will be

$$T = 288 \times 250 \times 0{\cdot}0166 = 1195{\cdot}2 \text{ tons.}$$

The co-efficient 0·0166 is found in Table IV, for the exponent of the displacement 2·375.

The last column, i, Tables II and V, contains the index for the length u of a parabola or ellipse.

$$u = \sqrt[i]{b^i + l^i}.$$

The corners on the diagrams are rounded in, to guard the ordinate lines from the centres.

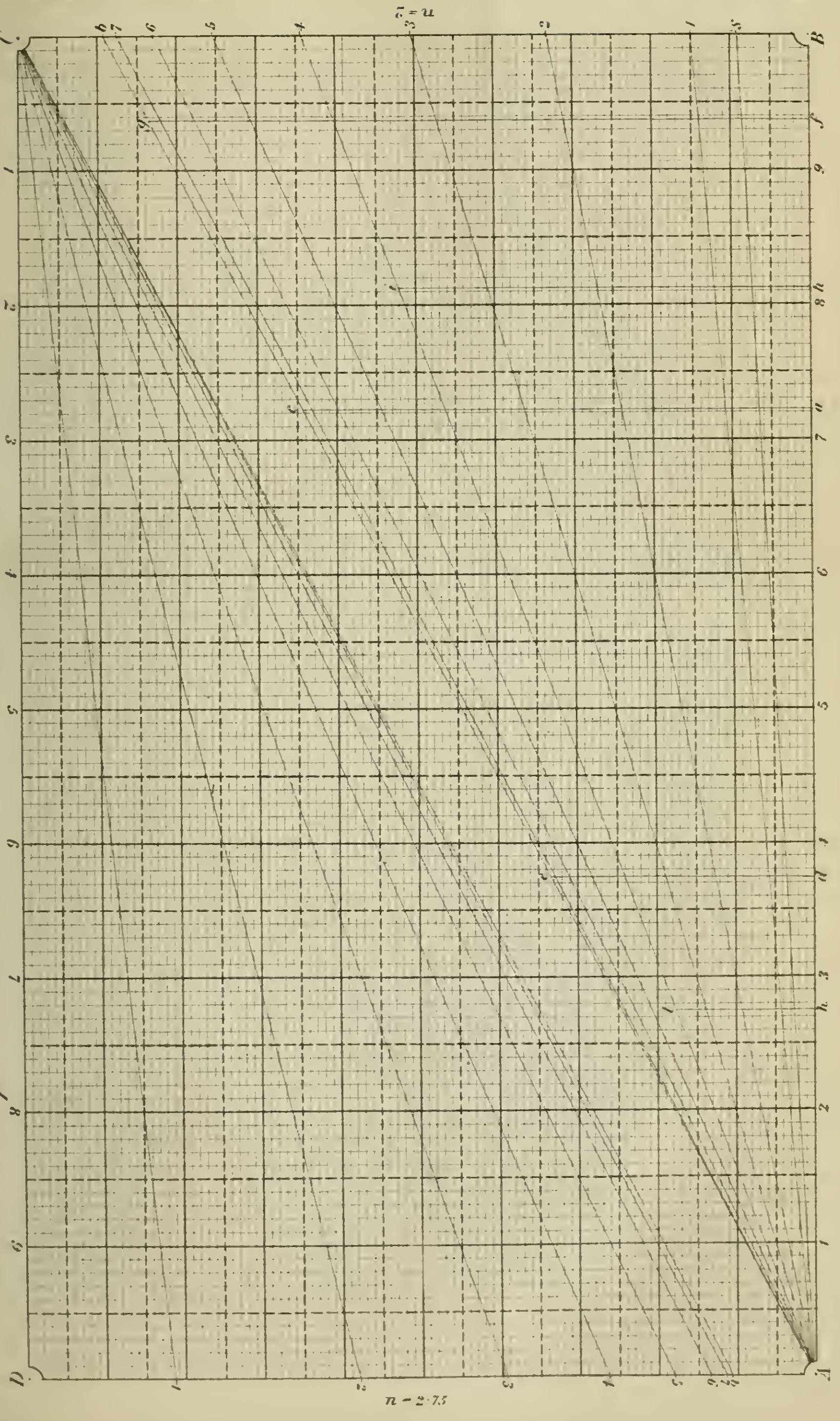
When the shipbuilder becomes accustomed to select the exponents, he can set his constructor or draftsman at work to construct just exactly what he wants, without his immediate attention.

The Pneumatic Despatch.

From the London Builder, No. 1069.

The report to be presented to the third ordinary general meeting of the company, having referred to the removal of the experimental tube and machinery from Battersea, and its having been laid underground from the Euston Station of the London and North-Western Company to the district post-office in Eversholt Street, a length of 600 yards, states that on the 20th February last, the Post-office authorities discontinued their street conveyances, and entrusted the company with the transmission of the mails, and that the service of the district had

Nyström's Parabolic Construction of Ships.



since been entirely performed by the company. Thirty trains per diem (Sundays excepted) have been despatched with perfect regularity, and upwards of 4000 trains have run without impediment or delay. The time occupied in the transmission has not exceeded 70 seconds. The daily cost of working has averaged £1 4s. 5d.; and five times the number of trains could have been conveyed without any appreciable increase of expense.

DECISIONS IN PATENT CASES.

Reported by H. Howson, Esq., Philadelphia.

Decision of the Examiner.

In the matter of the Interference between the application of B. H. Lightfoot for mode of Treating Tanned Leather, and the application of A. H. Eastman for a similar invention. C. F. Stansbury, Esq., Counsel for A. H. Eastman; H. Howson, Esq., Counsel for B. H. Lightfoot.

U. S. Patent Office, July 27, 1863.

To the Commissioner of Patents:

Sir:—I have the honor to submit herewith my report and decision in the matter of interference between the application of Benjaman H. Lightfoot, for mode of treating tanned leather, and the application of Alexander H. Eastman for a similar invention.

The invention in this case consists in producing a new composition to be used in that treatment of leather technically called "dubbing," the novelty consisting in substituting the oily matters derived from Petroleum for the animal oils commonly used for that purpose; there is some difference between the inventions as described and claimed by the two inventors, but the difference does not appear to be of much importance. The samples of leather produced are all of very nearly the same character. Eastman claims the application of the residuum of Petroleum treated as herein described, to the finishing of leather as a substitute for the oils now used for that purpose, all as set forth in this specification; he obtains his oils by treating the residuum left in the still. After the distillation of Petroleum, the oil so prepared is combined with tallow, and used for dubbing.

Lightfoot claims "the treatment of tanned leather by the application to the same, substantially in the manner described, of Petroleum or any oily hydrocarbons, holding paraffine in solution in combination with tallow or its equivalent."

From this statement it is clear that he meant to embrace the invention described by Eastman in general terms, although he does not describe the exact mode of preparation or the same materials with which to prepare it as set forth by Eastman. Were it shown that Eastman's preparation was a better or cheaper material, there might be grounds for allowing its patentability independently of Lightfoot's invention. But the testimony of the experts shows that there is no essential difference between the compositions described by Lightfoot and that of

Eastman; besides, Lightfoot states that the heavy oil from Petroleum such as used for lubricating machinery may be used with the best effect. The inventions are therefore, as far as appears from the testimony, substantially the same, and the interference was properly declared.

A large amount of testimony was produced on each side, and the case appears to be fully and fairly presented for decision; the inventors where wholly independent of each other, and each was no doubt perfectly unaware what the other was doing. The experiments of both extended over a considerable period of time, sufficient in fact to show that neither manifested much diligence, probably from the fact that neither fully appreciated the value of the invention he was making. Both parties made the invention and reduced it to practice, so that either would have been able to obtain a valid patent had the other not applied.

Eastman made oath to this invention on the 20th, and Lightfoot on the 30th day of January, 1863.

The testimony for Eastman shows that he began to use some kind of Petroleum product for greasing leather in January or February, 1861, but the first distinct mention of the invention in dispute was in March, 1862. The witnesses Huff, Blanchard, and Horton, prove that Eastman had at that time made a practical use of his invention, and the invention is that claimed by Lightfoot, that is the combination of a hydrocarbon with tallow, for a leather dressing. Eastman's other witnesses, as Taylor and Horton, mention the use of some preparation from Petroleum for oiling belts, boots, &c., during the months of January, February, July, and September, 1861. This was at the oil wells in Pennsylvania, but as there is no mention of the use of tallow with the oil, or any application to undressed leather, this testimony is not material to the issue, except to show that Eastman was at that time making the preliminary experiments which led to his invention.

On behalf of Lightfoot it is shown that in September, 1860, he sent a bottle of coal oil to Bush and Clark at Wilmington, Delaware, who were engaged in tanning and currying leather, to be used in dubbing leather. But no application was made of this oil to leather until 1863. William Lightfoot says, that in the latter part of April, or 1st of May, 1861, B. H. Lightfoot showed him a piece of leather that had been curried or dressed with coal oil, and also a sample of oil marked "N." The witness, Chas. Toppan, says that between the 22d and 26th of March, 1861, he was in the office of Lightfoot in Philadelphia, when the latter showed him some new leather and some oil, the oil to be used instead of "Straits" or "Bank oil," in the stuffing of leather; Toppan remembered what was told him by Lightfoot, and in the latter part of the summer of 1861, called the attention of curriers to the use of coal oil for stuffing, and sold for that purpose a considerable quantity of oil.

It appears that during the months of March, April, and May, 1861, Lightfoot had a very distinct idea of his invention, and made experiments sufficient to test its efficacy. Why the matter was not followed up more closely does not appear. But for the aid of Toppan there is no probability that the invention would have been tested on a large scale. It was however so tested by the persons to whom Toppan suggested the use of Petroleum products as substitutes for Straits or Bank oil.

The sum of the testimony for Lightfoot is, that in September, 1860, he sent a sample of coal oil to a currier to be tested. But no test was made, and the currier did not believe it would answer the purpose intended. In March, 1861, he applied the oil to a sample of new leather, and showed it to Toppan, and at the same time explained the invention to Toppan. In April or May, 1861, Lightfoot showed a sample of oil and a specimen of leather which had been dressed with the oil to his brother; from this time it does not appear that Lightfoot did anything until he made his application for a patent in January, 1863.

The sum of the testimony for Eastman is, that in the early part of 1861, he used thick oil for greasing belts, boots, &c., that is, for softening leather. In March, 1862, he applied a stuffing of tallow and heavy oil to a quantity of new leather; in July, 1862, he had some other sides of leather dressed with the same material. The experiments seemed to have been satisfactory; no reason is assigned for the delay in applying for a patent.

There is no doubt that Eastman made and applied the invention as early as March, 1862. There is no doubt that Lightfoot conceived the invention as early as September, 1860, but did not apply it on a working scale till quite recently. But his communication of the invention to Toppan in March, 1861, was such as to enable him to impart the invention to others skilled in the art, and it was so imparted, and it was used by curriers many months before either party made application for a patent.

Although Eastman's patent had been ordered to issue before Lightfoot's application was filed, the interval between the filing of the two applications was only nine days.

After a careful examination of the testimony produced, I am of the opinion that Lightfoot first made the invention in dispute, and I decide the question of priority of invention in his favor, and recommend that a patent be granted to him for his claim as it now stands.

Could it be shown on the part of Eastman that the particular composition claimed by him possesses properties which make it superior to the preparations made from the ordinary heavy oils as described by Lightfoot, a subordinate patent might be allowed to him for his composition as described and claimed; unless such superiority can be shown, I recommend that Eastman's application be rejected.

B. S. Hedrick, *Examiner.*

Decision of the Board of Examiners in Chief.

U. S. Patent Office, 12th October, 1863.

Interference between the application of Benjamin H. Lightfoot for a Patent for Dressing Leather, and the application of Alexander H. Eastman for a like patent.

On appeal to the Examiners in Chief, Eastman endeavors to secure to himself the monopoly of "the application of the residuum of Petroleum, treated as described in his specification" to the finishing of leather as a substitute "for the oils previously employed for that purpose. His specification seems to require as a prerequisite to the proposed use of Petroleum, that it should be treated with certain acids and alkalis, in a way which he describes."

Lightfoot's specification claims exclusively "the treatment of tanned leather by the application to the same, substantially in the manner described of Petroleum, or any oily hydrocarbon, holding paraffine in solution, in combination with tallow or its equivalent." As Eastman contemplates mixing his oil with tallow, after it is prepared according to his instructions, before applying it to the hides, the propriety of declaring the interference is manifest. It is true that Eastman uses no oil but such as has been treated according to this method, and Lightfoot uses Petroleum in all its forms.

But if Eastman was the first to discover his method, Lightfoot's patent if he can have one at all, should be made subordinate upon the face of it to that of Eastman. And the same remark may be made respecting Eastman's, should Lightfoot be considered the earliest in the race of discovery. Lightfoot shows that he had conceived the idea of substituting Petroleum, or some of its products, for the oils used in curing leather, as early as in September, 1860. At that date he actually described the process to others, and made an application of some of the oil to harness. This cannot, however, be regarded as reducing his theory to practice, nor even as demonstrating its practicability. It merely showed that the oil would soften leather, which had become hard after having been tanned.

But this must have been well known to the operatives at the oil wells, from the time when they were first worked. In order to show how hides in the process of tanning would be affected, it was necessary to first stiffen the oil with tallow, or something equivalent, so that the mixture would adhere to the skins when hung up, and then to apply it to hides, which had been partially tanned. Whatever Lightfoot may have been doing meanwhile, there is no evidence of his having made any further progress in his discovery until March, 1861. He then exhibited a piece of leather to a dealer in oils, Charles Toppan, and at the same time informed him that it was dressed with Petroleum oil.

The last of April or 1st of May after, he showed a piece to his brother William Lightfoot, and made the same remark about it. Mr. Toppan testifies further that the sample exhibited to him, corresponded precisely to other leather, which he produced upon his examination, and knew to have been dressed with Petroleum oil. At that time Lightfoot appears to have been entirely ignorant that any one else was prosecuting the same course of inquiry, and manifested no eagerness to establish his priority. His remarks, taken in connexion with his exhibition of the sample of his work, may fairly be considered as a part of the *res gestæ*. And the evidence of the two witnesses proves satisfactorily that in March 1861, Lightfoot had matured his discovery, had shown its feasbility, and had reduced it to actual practice. Nothing was wanting to render the invention complete on his part.

Eastman is a tanner and currier by trade, but had become interested in the oil wells in Pennsylvania in January, 1861, and was engaged in carrying one of them on. His mind seems to have been at once struck with the idea that the oil might be substituted for that which he had been accustomed to use in stuffing leather. He imparted the idea directly to several of his associates, and in order to demonstrate its correctness, made various applications of it to leather which had been previously dressed. For the reasons given above, these experiments cannot be regarded as having reduced his theory to any practical form. Before experimenting directly upon hides, or anything but leather already tanned, and before preparing the oil with tallow or the like, he seems to have supposed that the oil must first be subjected to some purifying process, and to have addressed himself entirely to perfecting such a process. At any rate, there is no evidence that he ever made any application of the oil to hides in the process of tanning before March, 1862, when two tanners, Huff and Blanchard, at his instance, used the oil in lieu of the fish oil ordinarily employed in tanning some skins. Upon the principles heretofore laid down, this is the earliest date at which he can be said to have so far matured his invention as to have entitled himself to a patent; and it clearly places him far behind Lightfoot.

As Lightfoot's counsel places much reliance upon Toppan's having induced the tanners with whom he dealt, to use this oil for stuffing hides, and upon his having sold them large quantities for the purpose, it is proper to remark that Toppan does not carry this back any further than nine months previous to his testifying which was in March, 1863. It is evident that nothing of the kind had been done by him previously to July, 1862, when we wrote to Lightfoot on the subject, but failed to elicit any reply.

It is objected to Lightfoot's application that by his long delay in presenting it he has lost his right. This delay does not amount to two years. Besides that, his failure in business and straightened circumstances constitute in the opinion of this Board, a fair answer to the objection. That he paid debts to a large amount, if it were more clearly shown, does not weaken his claims, but rather entitled him to favorable consideration. It was honorable in him to appropriate his scanty means to such a purpose, before embarking in an undertaking which must have been considered hazardous.

The decision of the primary Examiner, adjudging Lightfoot to be the prior inventor, and rejecting Eastman's application, is therefore affirmed.

J. H. Hodges,
J. J. Coombs,
Examiners in Chief.

Mr. Eastman after this decision withdrew his claims, and the patent dated November 17, 1863, was granted to B. H. Lightfoot.

For the Journal of the Franklin Institute.

Investigation of Formulæ for Determining the proper size of Shafts and Piston Rods. By D. M. Greene, 2d Assistant Engineer, U.S.N.

An article published in a recent number of the *Journal* contains several useful formulæ for determining the proper sizes of various important parts of the steam engine; but as these formulæ are not accompanied by any investigations or explanation of the processes by which they were obtained, they fail to meet the requirements of the intelligent and careful designer of steam machinery.

It is the purpose of the present paper to develop formulæ for calculating the diameters of *Shafts* and *Piston Rods*,—divesting the investigations of such technicalities as would tend to embarrass the general reader.

Shafts.—*Wrought Iron.—The moment of a pressure tending to produce rotation about an axis, or to resist such motion, is equal to the intensity of the pressure or resistance multiplied by the lever arm with which such pressure or resistance acts.*

In order to determine the relation between the diameters of shafts and their moments of resistance to torsional strain,

Let $s =$ torsional strength per square inch of transverse sectional area of shaft.

a, a_1, a_2, &c., = the sectional areas.
$d, nd, n_1 d =$ their diameters.
$r, nr, n_1 r$, &c., = their lever arms of resistance.
m, m_1, m_2, &c., = their moments.

Then,

$$m = a\,s\,r \qquad m_1 = a_1\,s\,nr$$

whence

$$m : m_1 : : a\,r : a_1\,nr. \qquad (1)$$

$$\text{But, } a = \cdot 7854 d^2 \quad a_1 = \cdot 7854 n^2 d^2$$

Substituting these values in (1) we get

$$m : m_1 : : \cdot 7854 d^2 r : \cdot 7854\, n^2 d^2\, nr : : 1 : n^3.] \qquad (2)$$

Similarly,

$$m : m_2 : : 1 : n_1^3 \qquad (3)$$

From (2) and (3) we get

$$m_1 : m_2 : : n^3 : n_1^3 \qquad (4)$$

Hence, the moments of resistance of shafts to torsional strain vary directly as the cubes of their diameters.

If $d =$ one inch in the preceding notation, $1, n, n_1, n_2$, &c., will represent the number of inches in the diameters of the shafts.

If, again, $n = 1$, (4) becomes

$$m_1 \,;\, m_2 : : 1 : n_1^3 ;$$

whence

$$m_2 = m_1\, n_1^3 ; \qquad (5)$$

Or *the moment of resistance to torsion of a shaft whose diameter is n_1 inches, is equal to the moment of resistance of a shaft whose diameter is one inch multiplied by n_1^3.*

If now the value of m_1 for a shaft one inch in diameter be determined experimentally, the moment of resistance of any other shaft is readily determined.

The following is the result of an experiment made for the purpose of determining the value of m_1.

"*A square bar of wrought iron (Ulster Iron Co.'s,) with a journal one inch in diameter and one-fourth inch in length, twisted with* 326 *pounds and broke with* 570 *pounds applied at the end of a lever* $2\frac{1}{2}$ *feet in length.*"

Now a weight of 570 lbs. acting with a lever arm of $2\frac{1}{2}$ feet is equivalent to $570 \times 2\frac{1}{2} = 1425$ pounds acting with an arm of 1 foot = 1425 lbs. ft.

The value just obtained would be the value of m_1, if we use the ultimate strength of the iron; but in practice, to insure safety, it is customary to use only about *one-sixth*, which would give $\frac{1425}{6} = 237{\cdot}5$ for m_1; since, however, the iron used in the experiment above was of a superior quality and strength, we shall take

$$m_1 = 200 \quad . \quad . \quad . \qquad (6)$$

Resuming the use of d for the diameter of any shaft in inches; and putting for m_1 the value just found, eq. (5) becomes

$$m = 200d^3; \quad . \quad . \quad . \qquad (7)$$

m being the moment of safe resistance in pounds feet of the shaft whose diameter is d.

If P = the pressure on the crank pin,
and a = the length of the crank arm,

then will,

$$m^1 = P\,a, \quad . \quad . \quad . \qquad (8)$$

be the moment of pressure, which must be equal to the moment of resistance of the shaft: or, (7) and (8) (taking the direction of P parallel to the piston rod),

$$200d^3 = P\,a \quad . \quad . \quad . \qquad (9)$$

Now, to find the value of P in terms of the dimensions of the cylinder and the pressure upon the piston,

Let D = the diameter of the cylinder in inches.
S = the stroke in feet $= 2\,a$
p = effective pressure in lbs. per square inch on the piston (maximum).
= the boiler pressure per steam gauge + vacuum.

Then will

$$P = {\cdot}7854D^2p, \qquad a = \frac{S}{2},$$

and $P\,a = \frac{{\cdot}7854}{2}D^2ps$; which in (9), gives

$$200d^3 = \frac{{\cdot}7854}{2}D^2ps,$$

whence,

$$d^3 = \frac{{\cdot}7854}{400}D^2ps$$
$$= {\cdot}002\ D^2ps.$$

$$\therefore\ d = \sqrt[3]{\cdot 002\text{D}^2 ps.} \qquad . \qquad . \qquad (10)$$

for the shaft of a single engine.

For a double engine, with the cranks placed at right angles, we have as before

$$200d^3 = \text{P}^1 a^1, \quad . \quad . \quad . \qquad (11)$$

in which P^1 represents the pressure upon the two crank pins, and a^1 the lever arms with which they act, when the strain upon the shaft is a maximum; which will be when each crank makes an angle of 45° with the direction of the piston rod.

In this position we shall have,

$$a^1 = a \cos. 45^\circ$$
$$= \frac{\text{s}}{2} \times \cdot 7$$

and since there are two cylinders, we shall have, $\text{P}^1 = 2 \times \cdot 7854\text{D}^2 p$;

$$\text{and } \text{P}^1 a^1 = \frac{2 \times \cdot 7 \times \cdot 7854\,\text{D}^2 ps.}{2}$$

This value substituted in (11) gives

$$200d^3 = 2 \times \cdot 7 \times \cdot 7854\,\text{D}^2 ps.$$

whence,

$$d^3 = \frac{2 \times \cdot 7 \times \cdot 7854}{400}\,\text{D}^2\, ps.$$
$$= \cdot 0027\,\text{D}^2 ps.$$

$$\text{and} \qquad \sqrt[3]{\cdot 0027\,\text{D}^2\, ps.} \qquad . \qquad . \qquad (12)$$

For an engine with three cylinders, having its cranks placed at angles of 120°, we shall have the maximum torsional strain on the shaft, when one crank is perpendicular to, and the two others each make an angle of 30° with the direction of, the piston rod.

In this case we shall have

$$200d^3 = \text{P}\,a + \text{P}\,a \sin. 30^\circ + \text{P}\,a \sin. 30^\circ$$
$$= \frac{\text{P}\,s}{2}(1 + 2 \sin. 30^\circ).$$
$$= \text{P}\,s.$$
$$= \cdot 7854\,\text{D}^2\, ps.$$

whence,

$$d^3 = \frac{\cdot 7854}{200}\,\text{D}^2\, ps.$$
$$= \cdot 0039\,\text{D}^2\, ps.$$

$$\text{and} \qquad d = \sqrt[3]{\cdot 0039\,\text{D}^2\, ps.} \qquad . \qquad . \qquad . \qquad (14)$$

Comparing now (10), (12), and 14), supposing D, p, and s constant, we get

$$d_1 : d_2 ; d_3 : : \sqrt[3]{20} : \sqrt[3]{27} : \sqrt[3]{39}$$
$$: : 1 : 1{\cdot}1 : 1{\cdot}25;$$

whence it appears that diameters of the shafts of single, double, and treble engines, having cylinders of the same size and using the same pressure of steam, are to each other respectively as 1, 1·1 and 1·25.

Recapitulation.

For Single Engine (10).

$$d=\sqrt[3]{\cdot 002\text{D}^2\,ps}.$$

For Double Engine (12).

$$d=\sqrt[3]{\cdot 0027\text{D}^2\,ps}.$$

For Treble Engines (14).

$$d=\sqrt[3]{\cdot 0039\text{D}^2\,ps}.$$

PISTON RODS.—(*Wrought Iron*).—For all the knowledge we possess in relation to the resistance of columns to crushing strains we are directly indebted to experiment.

It is found that columns of wrought iron will bear without crushing, per square inch:—

When their lengths are less than 12 diameters,	14,225 lbs.
" " " " between 12 and 24 "	11,877 "
" " " " greater than 24 "	7,112 "

In practice we may take one half these values.

Let s = the safe resistance of the material per square inch.
d = diameter of the piston rod in inches.
a = area of transverse section in square inches.

Then we shall have for the pressure which any rod will bear with safety

$$\text{P}=a\,s$$
$$=\cdot 7854d^2\,s.$$

Substituting now in this expression, for s, the safe resistances indicated above, we get,

$$\text{P}=\frac{14225}{2}\times\cdot 7854d^2 \quad . \quad . \quad (1)$$

$$\text{P}_1=\frac{11877}{2}\times\cdot 7854d^2 \quad . \quad . \quad (2)$$

$$\text{P}_2=\frac{7112}{2}\times\cdot 7854d^2 \quad . \quad . \quad (3)$$

These expressions must be made equal to the pressure upon the pistons. To obtain an expression for the effective pressure upon any piston:—

Let D = diameter of cylinder in inches.
p = effective pressure per square inch upon the piston = pressure per steam gauge vacuum.

Then we shall have for the whole pressure on the piston:—

$$\text{P}=\cdot 7854\text{D}^2p.$$

Equating successively the values in (1), (2), and (3) with the preceding, we get

$$1^\circ. \qquad \frac{14225}{2}\times\cdot 7854d^2=7854\ \text{D}^2p.$$

whence $$d^2 = \frac{2 \times \cdot7854}{14225}\text{D}^2p$$

and $$d^2 = 0\cdot11\text{D}\sqrt{p} \quad . \quad . \quad . \qquad (4)$$

2. $$\frac{11877}{2} \times \cdot7854d^2 = \cdot7854\,\text{D}^2p.$$

whence $$d^2 = \frac{2 \times \cdot7854}{11877}\text{D}^2p$$

and $$d = \cdot013\text{D}\sqrt{p}. \quad . \quad . \qquad (5)$$

3. $$\frac{7112}{2} \times .7854d^2 = \cdot7854\text{D}^2p$$

whence $$d^2 = \frac{2 \times \cdot7854}{7112} \times \text{D}^2p.$$

and $$d = \cdot017\text{D}\sqrt{p} \qquad (6)$$

Steel Rods.—The relative abilities of wrought iron and steel to resist crushing strains are as 1745 to 2518; hence, the transverse sectional areas of piston rods made of these materials, should be inversely proportional to these numbers; or their diameters should be inversely proportional to the square root of of 1745 and 2518.

Now, $$\sqrt{\frac{1745}{2518}} = \cdot83$$

and (4) becomes $$d = \cdot83 \times \cdot011\text{D}\sqrt{p}$$
$$= \cdot009\text{D}\sqrt{p} \quad . \quad . \qquad (4)^{a}$$

Similarly, (5) becomes $$d = \cdot83 \times \cdot013\text{D}\sqrt{p}$$
$$= \cdot011\text{D}\sqrt{p} \quad . \quad . \qquad (5)^{a}$$

and (6) becomes $$d = \cdot83 \times \cdot017\text{D}\sqrt{p}$$
$$= \cdot014\text{D}\sqrt{p} \quad . \quad . \qquad (6)^{a}$$

Recapitulation.

When	Wrought Iron.	Steel.
$l < 12d$,	$d = \cdot011\text{D}\sqrt{p}$	$d = \cdot009\text{D}\sqrt{p}$
$l > 12d$ and $l < 24d$	$d = \cdot013\text{D}\sqrt{p}$	$d = \cdot011\text{D}\sqrt{p}$
$l > 24d$	$d = \cdot017d\sqrt{p}$	$d = \cdot014\text{D}\sqrt{p}$

l being the length of the rod.

If different values be assigned to p in the above formulæ, tables may be readily constructed. The three cases taken, correspond to *short*, *medium*, and *long* strokes. Hence the formula for d when $l < 12d$ would apply to the piston rods of propeller engines; but since the speed of piston in the propeller engine is considerable, it would perhaps be better to use the formula (5) or $(5)^{a}$, in determining the diameters of their rods; in any other case, a mental comparison between the length and diameter of the cylinder will show which formula should be used.

Naval Academy, Newport, R. I., Dec. 9, 1863.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, December 17, 1863.

John C. Cresson, President, in the chair.

John Agnew, Vice President.

Robert Briggs, Recording Secretary, *pro tem.*

The minutes of the last meeting were read and approved.

A letter was read from the Royal Society, London.

Donations to the Library were received from the Royal Society, and the Society of Arts, London; la Société Industrielle de Mulhouse, France; the Oesterreichischen Ingenieur-Vereines, Vienna, Austria; the Commissioner of Agriculture, Washington, D. C.; Prof. John F. Frazer, Frederick Graff, Esq., and John Warner, Esq., Philadelphia.

A donation to the Cabinet of Models, from Rufus A. Wilder, Esq., Schuylkill Haven, Pennsylvania.

The Periodicals received in exchange for the Journal of the Institute were laid on the table.

The Treasurer's statement of the receipts and payments for the month of November was read.

The Board of Managers and Standing Committees reported their minutes.

Candidates for membership in the Institute (12) were proposed, and those proposed at the last meeting (24) were duly elected.

Nominations were made for Officers, Managers, and Auditors of the Institute for the ensuing year.

Prof. Fleury submitted specimens of the incombustible papier-mache moulds used by Prof. Vander Wayde in his process of stereotyping.

Mr. John W. Nystrom said:—Since the last meeting I have made some inquiries about paper moulds for stereotyping, and as far as I am able to trace it back, it appears to be an old American invention, experimented upon and brought to some perfection by the late Mr. L. Johnson, of this city, some thirty years ago; the process has since been sold to Mexico. It has been gradually improved, and lately brought to greater perfection. About five years ago an Italian in this country made a great improvement in the art, by inventing an incombustible solution for the paper mould, consisting of a strong glue mixed with alum and plaster of paris, and the process is now in successful operation in different parts of the world. It is recommended by R. Hoe & Co., and now employed on twelve of their printing presses, of which the Philadelphia Inquirer is one, and the only one in this city. The New York Herald, Tribune, and Times are printed from plates cast in paper moulds; three papers in London, one in Manchester, and one in Paris. Paper moulds are also employed for book printing in Manchester. I have brought to the meeting one stereotype plate and paper moulds from which to-day's Inquirer was produced, and contribute the same to the Cabinet of the Institute. The plate, as you see, is cylindrical-convex on the face, made to fit the cylinder press, which latter makes 20,000 impressions per hour. It requires only sixteen minutes to mould, cast, and finish the stereotype plate. I have here also samples of plates from the Smithsonian Institution, which may be of interest to printers. This plate is cast from a mould made of two parts of China clay and one part of powdered soapstone, mixed with water to a tough dough, spread out very thin on an iron plate, and the type form is pressed into it. The other plate is made of clay, instead of type metal; the Catalogues of the Smithsonian Institution are printed from such plates, made up of small pieces, one for each book, which can at any time be arranged in any desired order; such plates are very cheap, but do not admit of alterations. I have here also a sample of an electrotype plate from my Pocket Book.

Mr. Fleury then read the following paper:

Carbonic Acid, Chlorine, Fluorine, Hydrogen, Sulphurous, and Sulphuretted Hydrogen Gases have already taken a prominent place in our industrial pursuits, and it gives me much pleasure to be able to present to you this evening a new device for producing these gases, by a very simple and cheap apparatus, the invention of one of our members, Mr. Frederick M. Ruschhaupt, of Berlin, Prussia.

I now exhibit to you two modifications of this apparatus, one of a very simple construction, which conveys the gas by its own pressure to the place where it is required; the other, an arrangement for a continuous equal pressure, and which can at all times be obtained, an advantage that no other apparatus possesses. These machines can be made, the one constructed to convey gas by its own pressure at $75—the one which gives a continuous equal pressure at $150—while the machines now in use cost at least $300. These apparata are made of wood and lead (or other suitable materials), and are sufficiently strong for all practical purposes. Their construction is such that the evolution of the gas can be stopped at any moment, and the apparatus can be cleaned without incurring any danger.

The arrangement whereby any desired pressure can be obtained and kept up is a very valuable one. For the numerous applications of such gases as carbonic acid, where it is sometimes very important to keep up the same pressure, this machine is admirably adapted. For the manufacture of soda and other mineral waters, for the new system of baking aërated bread, for the laboratory, as well as for the hospital and private physician, these machines can be well recommended.

By some modification, other gases, such as chlorine and sulphuretted hydrogen, can be evolved cheaply and effectually. The machine suggests other applications, as for instance, the preparation of hydrogen and carburetted hydrogen gases, *ad libitum*, for the production of light and heat. The preparation of hydrofluoric acid for etching on glass, or for dissolving of silica, may also be safely, easily, and cheaply effected by these same machines.

Those who desire further information may address Mr. G. F. Kershaw, Attorney, No. 324 Chestnut Street, in whose hands the American patent and all papers relating to the subject have been placed by the inventor.

Prof. Fleury read the explanation of the advantages of Dr. Vander Weyde's patent apparatus for the making and administering of Nitrous Oxid Gas, for anæsthetic purposes.

The administration of nitrous oxid, or so-called laughing gas, during short but painful operations, cannot be enough recommended; in using it, we avoid entirely the shock produced on the system by intense pain, a shock especially injurious to persons of a delicate nervous temperament.

This gas being itself a supporter of combustion and of life, does not produce any unpleasant effects when administered properly, much less has it ever proved fatal; a fact which by no means can be stated about chloroform, which far from supporting life extinguishes it, when used in large doses, as has been proved by numerous fatal results, consequent to its administration.

It is a singular fact that among the three anæsthetics now in use, ether, chloroform, and nitrous oxid, the first is combustible itself, the vapor being just the opposite of a supporter of combustion and life; the last, nitrous oxid, is a powerful supporter of combustion and of life, and chloroform is neither combustible nor a supporter of combustion.

Ether therefore is worse than chloroform; its vapor is directly suffocating, and its effect is due to neither more nor less than a partial suffocation, which very easily may be carried too far, and often has been carried too far; this accounts for its unpleasant after effects, even when administered in a moderate dose.

Professor Vander Weyde has come to this conviction not only by the study of the chemical properties of ether, but by the observation of its physiological effects, made during several years in the New York hospitals and medical colleges; for this reason he is of opinion that only two anæsthetics should be used, namely, nitrous oxid and chloroform.

The first acting very rapidly and its effects lasting only a short period of time, is appropriate for all surgical operations of little duration, like most of those required in dentistry; the latter (chloroform) acting slower and its effects being more lasting, is only suitable when a prolonged effect is desired, as in capital amputations, and, in general, all protracted surgical operations.

All anæsthetics act on the system similar to alcohol, the smallest effective dose produces exhilaration, a little more great excitement, which in different individuals manifests itself in a great variety of ways, a greater dose produces sleepiness, and the maximum dose perfect insensibility (total drunkenness). The difference between nitrous oxid and the other anæsthetics is, that the first being a supporter of combustion and respiration, stimulates the nervous system, and produces an increase of vitality, the others being non-supporters of combustion and respiration, depress the nervous system and bring vitality below the standard, both with the same final result, *perfect unconsciousness;* the difference only is that the unconsciousness produced by the increase of vital action is harmless, and the same result produced by the depression of vital action is injurious, and may prove fatal; in other words, that the excitement or unconsciousness following the use of nitrous oxid is harmless in its results, and the excitement or unconsciousness following the use of ether or chloroform is connected with danger.

It is a fact stated by Dr. Colton, who has administered the nitrous oxid to about 20,000 persons, more individuals than any other man living, that never any fatal result has followed; in the meantime, it is estimated that upwards of 3000 lives have been lost during the administration of ether and chloroform.

Since the nitrous oxid has been introduced for the above-mentioned purposes, it has been observed that in some instances persons after having used it, felt nausea and even vomited; now this is by no means the fault of the gas, but of the method of administering it.

Thus far it has in fact always been administered in a most disgusting manner; an india-rubber bag or bladder, of four, six, or eight gallons contents, provided with a stop-cock and mouth-piece, is filled with the gas and placed to the mouth of the patient, who has to breathe out and in the bag: as now during respiration the lungs give continually off carbonic acid, which is thrown out of the system by the act of expiration, as an injurious poisonous substance; this expired carbonic acid goes in the bag, mingles with the gas, dilutes it and makes it impure; more than that, it poisons it; all breathing in and out of a bag causes us to breathe our own breath over again, which is nothing less than breathing a poisoned atmosphere, rendered continually more so by every expiration; any one may verify this by breathing out and in such a bag, when simply filled with atmospheric air, after a very few minutes giddiness and nausea are the result, and those are the regular symptoms of poisoning by carbonic acid.

To avoid this objection, the Professor first constructed a stop-cock with valves, which opened and closed in such a way that the exhaled gas was expelled in the atmosphere and lost; he found, however, that in this way the amount required to affect some individuals was so extravagantly large, and the bags and quantities necessary so enormous, that he soon gave this up as too expensive and troublesome.

In the same time, the manner in which the gas has been produced and preserved thus far is cumbersome and imperfect in the extreme. Nitrous oxid is developed by decomposing nitrate of ammonia by heat in a glass retort, producing thus nitrous oxid and water, which distils over. This operation is expressed by the following chemical formula:

Nitrate of Ammonia is

Ammonia, NH_4O, Nitric Acid, NO_5.

This is by the application of heat decomposed in its elements, 2 N (two atoms of nitrogen,) 4 H (four atoms of hydrogen) and 6 O (six atoms of oxygen,) but like in all similar decompositions, the elements are not separated uncombined, but at the moment of their separation enter in new combinations, namely, 4 of the 6 atoms of oxygen combine with 4 atoms of hydrogen and form 4 HO (four atoms of water), and the two remaining atoms of oxygen combine with the two atoms of nitrogen and form 2 NO (two atoms of nitrous oxid). The products of this decomposition are sometimes passed through a washing-bottle, and if this is done, the operation is well enough as far as this is concerned, but the objectionable features are the next.

The gas is passed in a common wooden barrel filled with water; it is passed in from below, so that a quantity of water equal to the quantity of gas introduced continually flows out, causing a waste of water at least equal to the volume of all the gas manufactured, not to speak of the trouble of managing this water, when used in a locality without water-works; the gas being very soluble in water, will for a large portion be absorbed by it, and when left in this state, the diminishing volume of gas will continually be supplied by atmospheric air, when this has occasion to enter somewhere, thus diluting it and spoiling its effects. Sometimes the gas is passed in a very large india rubber bag, or in an apparatus partially made of india rubber, to keep it ready for use; from this bag it is passed in the smaller ones mentioned above, intended for its administration.

Now it is a fact known by experience to all who have attempted to preserve different kinds of gases in such bags, that after some days they all lose their characteristic properties, they are nearly always after a longer or shorter period of time changed in atmospheric air, and this by an interchange of their atoms through the infinitely small pores of the bag, with the atoms of the surrounding atmosphere, this action is called *endosmose*, and operates always when two different gases or fluids are separated by some membrane, however air-tight this membrane may be when placed between two gases or fluids of the same kind.

Another objection against india rubber bags is that they all deteriorate by age sooner or later, according to the quality of the rubber; rubber is nearly always undergoing a slow process of oxidation, as is proved by the analysis of fresh and of old rubber, the last always containing considerable oxygen, the fresh little or none; how gases act on rubber is illustrated by the peculiar disagreeable odor rubber tubes acquire when used to conduct common illuminating gas; the nitrous oxid also communicates a particular odor to the rubber bags, which in some instances is quite repugnant.

This imperfect mode of manufacturing, and worse method of administering nitrous oxid, has induced the Professor to attempt the construction of an apparatus by which all those difficulties are overcome, the apparatus not only being a substitute for the disgusting breathing bags, but also for the cumbersome barrel, and large expensive india rubber bags.

The success of his apparatus exceeds his most sanguine expectations, as it fulfils the following requisites:

1. *Completeness;* nothing else is wanted; it consists of three parts, first, the generator and purifier; second, the receiver and preserver; and third, the breathing apparatus.

2. *Cheapness.* Its expense is not more than the india rubber bags, barrels, or retorts, and it will be cheaper in the end, because of its

3. *Durability.* Being all of glass, it does not deteriorate in the course of time like wooden barrels, and much more the expensive india

rubber bags, which in a few years become useless, an experience made sooner or later by all who use them.

4. *Economy when preserving it.* No gas whatever is lost or wasted; a measured amount of nitrate of ammonia is sufficient to fill the receiver or reservoir with gas, which being of glass and floating exactly balanced in another glass vessel over diluted sulphuric acid, no gas is lost by endosmose or absorption of the water; the Professor found by a long series of experiments that of all the fluids and solutions tested, diluted sulphuric acid absorbs the gas least, and keeps it in the purest state.

5. *Economy in administering it.* As the gas is not drawn off in small bags, to be partially wasted, but is breathed directly from the reservoir, and all the exhaled gas returns to it by an entirely different channel, during which passage it is purified, no gas whatever can possibly be lost, but every part of what has been produced is made available to produce anæsthesia.

6. *Purity of the administered gas.* The purification spoken of above, is accomplished by passing the exhaled gas along a different channel through a solution of caustic potassa, which will necessarily retain the carbonic acid, and only permit the pure nitrous oxid to return to the reservoir. The strong affinity of caustic potassa for carbonic acid is known to all chemists, who ascertain the correct amount of carbon in organic compounds, by this power of a potash solution to absorb completely all carbonic acid in an atmosphere passing through it.

7. *Economy of time.* The apparatus is always in working order, the retort being kept filled with a dose of nitrate of ammonia; it is everywhere so provided for safety, that the operator has only to ignite the lamp, and need not meddle with it afterwards, except extinguishing it in about five minutes when the gas reservoir is filled, in five minutes more the white fumes have disappeared, and it may be used if necessary. The Professor has shown this at the meeting of the Dental Association in the city of New York, on Wednesday, November 17, where he for the first time exhibited his apparatus, and had one of the persons present fairly under the influence of the gas ten minutes after he commenced developing it.

However, it is better to wait longer and let the gas rest for one or two hours, then it is sweeter and more palatable, and the apparatus is just as appropriate for keeping the gas any length of time, as for developing it at a few minutes' notice.

Mr. P. E. Chase said that he thought that Dr. Vander Weyde had secured economy in administering the gas, by a fearful risk of health. Although the solution of caustic potassa might prove an adequate purifier so far as the carbonic acid was concerned, he did not think it would remove any contagious or miasmatic influence that might be contained in the breath. The same gas being breathed over and over by an unlimited number of patients, the reservoir might become so impregnated with tuberculous or eczematous virus, that the risk from inhalation would be far greater than from any ordinary exposure to contagion.

Mr. Fleury replied that it can be proved that the passing of the exhaled gas through the caustic potassa deprives the same of any virus or other poisonous substance that might lead to contagion.

A Comparison of some of the Meteorological Phenomena of Nov., 1863, *with those of* Nov., 1862, *and of the same month for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10⅓′ W. from Greenwich. By James A. Kirkpatrick, A. M.

	Nov. 1863.	Nov. 1862.	Nov. 13 years.
Thermometer—Highest—degree, .	69·0°	71·0°	89·0°
“ “ date .	5th.	2d.	1st, 1860.
“ Warmest day—Mean,	60·50	61·67	72·30
“ “ “ date,	5th.	2d.	9th, 1857.
“ Lowest—degree, .	25·00	27·00	16·00
“ “ date, . .	30th.	7th & 8th.	25th, 1860.
“ Coldest day—Mean,	30·50	30·50	23·30
“ “ “ date, .	30th.	7th.	25th, 1860.
“ Mean daily oscillation,	14·37	13·40	13·46
“ “ “ range, .	6·35	5·94	5·70
“ Means at 7 A. M., .	42·45	40·67	41·17
“ “ 2 P. M., .	52·63	48·18	50·37
“ “ 9 P. M., .	46·12	43·25	44·40
“ “ for the month,	47·07	44·03	45·31
Barometer—Highest—Inches, .	30·249 in.	30·555 in.	30·661 in.
“ “ date, . .	23d.	16th.	12th, 1851.
“ Greatest mean daily press.	30·232	30·509	30·520
“ “ date, . .	23d.	16th.	12th, 1851.
“ Lowest—Inches, .	29·434	29·380	29·117
“ “ date, . .	17th.	20th.	19th, 1857.
“ Least mean daily press.,	29·498	29·467	29·255
“ “ date, . .	17th.	20th.	19th, 1857.
“ Mean daily range, .	0·185	0·159	0·184
“ Means at 7 A. M., .	29·863	29·877	29·914
“ “ 2 P. M., .	29·804	29·823	29·871
“ “ 9 P. M., .	29·854	29·870	29·901
“ “ for the month,	29·840	29·857	29·895
Force of Vapor—Greatest—Inches,	0·463 in.	0·548 in.	0·832 in.
“ “ “ date, .	15th.	20th.	8th, 1857.
“ “ Least—Inches, .	·074	·106	·055
“ “ “ date, .	10th.	16th.	25th, 1857.
“ “ Means at 7 A. M.,	·208	·213	·226
“ “ “ 2 P. M.,	·214	·228	·230
“ “ “ 9 P. M.,	·216	·224	·233
“ “ “ for the month.	·213	·222	·230
Relative Humidity—Greatest—per ct.,	97 per ct.	100 per ct.	100 per ct.
“ “ “ date,	15th.	7th.	Often.;
“ “ Least—per ct.,	25·0	37·0	25·0
“ “ “ date,	7th.	13th.	7th, 1863.
“ “ Means at 7 A.M.,	73·7	79·4	77·5
“ “ “ 2 P.M..	52·2	64·5	58·9
“ “ “ 9 P.M ,	66·0	75·1	73·6
“ “ “ for the month	64·0	73·0	70·0
Clouds—Number of clear days,* .	6	8	8·5
“ “ cloudy days, .	24	22	21·5
“ Means of sky cov'd at 7 A.M.	57·3 per ct.	64·0 per ct.	59·9 per ct.
“ “ “ “ 2 P.M.	54·3	64·3	60·0
“ “ “ “ 9 P.M.	52·3	63·7	52·5
“ “ “ for the month,	54·6	64·0	57·5
Rain and Melted Snow—Amount,	2·960 in.	4·455 in.	3·756 in.
No. of days on which Rain or Snow fell,	7	15	10·5
Prevailing Winds—Times in 1000,	N82°46′W·371	N 79° 0′W ·237	N70°41 W·262

* Less than one-third covered at the hours of observation.

A Comparison of some of the Meteorological Phenomena of the AUTUMN *of* 1863, *with that of* 1862, *and of the same Season for* THIRTEEN *years, at Philadelphia, Pa.*—Barometer 69 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	Autumn, 1863.	Autumn, 1862.	Autumn for 13 years.
Thermometer—Highest—degree, .	83·00°	87·00°	95·00°
“ “ date, .	7th Sept.	8th Sept.	Sep. 12, 1851.
“ Warmest day—Mean,	77·17	77·33	85·20
“ “ “ date,	7th Sept.	8th Sept.	Sept. 6, 1854.
“ Lowest—degree, .	25·00	27·00	16·00
“ “ date, . .	30th Nov.	Nov. 7 & 8.	Nov.25, 1860.
“ Coldest day—Mean,	30·50	30·50	23·30
“ “ “ date, .	30th Nov.	7th Nov.	Nov.25, 1860.
“ Mean daily oscillation,	15·04	14·98	15·33
“ “ “ range, .	5·17	5·24	5·29
“ Means at 7 A. M., .	51·27	52·30	51·67
“ “ 2 P. M., .	61·73	62·83	62·78
“ “ 9 P. M., .	55·00	55·93	55·49
“ “ for the Autumn,	56·00	57·02	56·65
Barometer—Highest—Inches, .	30·312 in.	30·555	30·661 in.
“ “ date, . .	23d Sept.	16th Nov.	Nov.12, 1851.
“ Greatest mean daily press.	30·244	30·509	30·520
“ “ date, . .	23d Sept.	16th Nov.	Nov.12, 1851.
“ Lowest—Inches, . .	29·281	29·307	29·012
“ “ date, . .	18th Sept.	27th Oct.	Oct. 26, 1857.
“ Least mean daily press.,	29·460	29·467	29·059
“ “ date, . .	18th Sept.	20th Nov.	Oct. 26, 1857.
“ Mean daily range, .	0·136	0·146	0·150
“ Means at 7 A. M., .	29·908	29·874	29·938
“ “ 2 P. M., .	29·862	29·831	29·896
“ “ 9 P. M., .	29·906	29·868	29·921
“ “ for the Autumn,	29·892	29·858	29·918
Force of Vapor—Greatest—Inches,	0·784 in.	0·833 in.	0·991 in.
“ “ “ date, .	17th Sept.	12th Sept.	Sept. 6, 1854.
“ “ Least—Inches, .	·074	·106	·055
“ “ “ date, .	10th Nov.	16th Nov.	Nov.25, 1857.
“ “ Means at 7 A. M.,	·311	·339	·338
“ “ “ 2 P. M.,	·324	·365	·357
“ “ “ 9 P. M.,	·328	·369	·357
“ “ “ for the Autumn,	·321	·358	·351
Relative Humidity—Greatest—per ct.,	97 per ct.	100 per ct.	100 per cent.
“ “ “ date,	15th Nov.	7th Nov.	Often.
“ “ Least—per ct.,	25·0	32·0	23·0
“ “ “ date, .	7th Nov.	3d Sept.	Oct. 21, 1859.
“ “ Means at 7 A.M.,	75·1	78·7	78·2
“ “ “ 2 P.M.,	53·8	59·4	57·1
“ “ “ 9 P.M.,	69·2	74·7	73·8
“ “ “ for Autumn,	66·0	70·9	69·7
Clouds—Number of clear days,* .	26	31	29·4
“ “ cloudy days, .	65	60	61·6
“ Means of sky cov'd at 7 A.M.	57·3 per ct.	58·8 per ct.	57·3 per ct.
“ “ “ “ 2 P.M.	55·6	55·3	55·4
“ “ “ “ 9 P.M.	44·5	52 8	42·6
“ “ “ for Autumn,	52·5	55·6	51·8
Rain and Melted Snow—Amount,	6·601 in.	14·897 in.	10·520 in.
No. of days on which Rain or Snow fell,	24	32	27·2
Prevailing Winds—Times in 1000,	N75°50′W·201	N64°56′W·140	N76°27′W·230

* Less than one-third covered at the hours of observation.

A Comparison of some of the Meteorological Phenomena of Dec., 1863, *with those of* Dec., 1862, *and of the same month for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By James A. Kirkpatrick, A. M.

	December, 1863.	December, 1862.	December for 13 years.
Thermometer—Highest—degree, .	60·00°	64·00	71·00°
“ “ date, .	14th.	15th.	2d, 1859.
“ Warmest day—Mean,	54·17	55·00	62·80
“ “ “ date,	13th.	15th.	2d, 1859.
“ Lowest—degree, .	15·00	8·00	4·50
“ “ date, .	23d.	21st.	19th, 1856.
“ Coldest day—Mean,	20·67	15·83	11·00
“ “ “ date, .	23d.	20th.	18th, 1856.
“ Mean daily oscillation,	13·26	12·74	12·35
“ “ “ range, .	6·54	6·46	6·38
“ Means at 7 A. M., .	30·87	32·24	31·70
“ “ 2 P. M., .	39·13	39·76	39·25
“ “ 9 P. M., .	34·68	34·71	34·59
“ “ for the month,	34·89	35·57	35·18
Barometer—Highest—Inches, .	30·495 in.	30·495 in.	30·678 in.
“ “ date, . .	7th.	20th.	18th, 1856.
“ Greatest mean daily press.	30·423	30·401	30·611
“ “ date, . .	7th.	20th.	18th, 1856.
“ Lowest—Inches, .	29·167	29·319	28·946
“ “ date, . .	14th.	16th.	9th, 1855.
“ Least mean daily press.,	29·341	29·507	29·175
“ “ date, . .	14th.	16th.	9th, 1855.
“ Mean daily range, .	0·223	0·197	0·212
“ Means at 7 A. M., .	29·999	29·932	29·962
“ “ 2 P. M., .	29·935	29·895	29·922
“ “ 9 P. M., .	29·981	29·936	29·947
“ “ for the month, .	29·972	29·921	29·944
Force of Vapor—Greatest—Inches,	0·486 in.	0·377 in.	0·551 in.
“ “ “ date, .	14th.	15th.	2d, 1859.
“ “ Least—Inches, .	·051 in.	·040	·025
“ “ “ date, .	11th.	20th.	18th, 1856.
“ “ Means at 7 A. M.,	·142	·154	·143
“ “ “ 2 P. M.,	·156	·163	·167
“ “ “ 9 P. M.,	·145	·162	·155
“ “ “ for the month,	·148	·160	·155
Relative Humidity—Greatest—per ct.,	96 per ct.	92 per ct.	100 per ct.
“ “ “ date,	14th.	14th.	Often.
“ “ Least—per ct.,	35·0	31	23·0
“ “ “ date, .	24th.	2d.	15th, 1861.
“ “ Means at 7 A.M.,	74·1	78·5	77·1
“ “ “ 2 P.M.,	59·8	61·5	65·3
“ “ “ 9 P.M.,	68·5	74·8	75·0
“ “ “ for the month	67·5	71·6	72·5
Clouds—Number of clear days,* .	10	13	9·1
“ “ cloudy days, .	21	18	21·9
“ Means of sky cov'd at 7 A.M.,	56·1 per ct.	52·6 per ct.	63·4 per ct.
“ “ “ “ 2 P.M.,	56·8	58·7	62·8
“ “ “ “ 9 P.M.,	47·4	42·6	46·1
“ “ “ for the month,	53·4	51·2	57·4
Rain and melted Snow—Amount, .	4·871 in.	1·555 in.	3·631 in.
No. of days on which Rain or Snow fell,	10	8	9·9
Prevailing Winds—Times in 1000,	N72°0′W ·243	N84°24′W ·301	N60°17′W ·285

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* Less than one-third covered at the hours of observation.

A Comparison of some of the Meteorological Phenomena of the year 1863, *with those of* 1862, *and of the last* TWELVE *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10⅔′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	1863.	1862.	12 Years.
Thermometer—Highest—degree, .	95·06°	95·50	100·50°
" " date, .	Aug. 10.	July 7.	July21, 1854.
" Warmest day—Mean,	88·50	87·67	91·30
" " " date,	Aug. 10.	Aug 9.	July21, 1854.
" Lowest—degree, .	5·00	8·00	— 5·50
" " date, .	Feb. 5.	Dec. 21.	Jan. 23, 1857.
" Coldest day—Mean,	11·17	15·83	— 1·00
" " " date. .	Feb. 4.	Dec. 20.	Jan. 9, 1856.
" Mean daily oscillation,	14·63	15·21	15·14
" " " range, .	5·39	5·15	5·55
" Means at 7 A. M., .	49·98	49·22	49·71
" " 2 P. M., .	59·21	59·00	59·90
" " 9 P. M., .	53·21	52·53	53·14
" " for the year,	54·13	53·58	54·25
Barometer—Highest—Inches, .	30·671 in.	30·555 in.	30·704 in.
" " date, . .	Feb. 4.	Nov. 16.	Jan. 28, 1853.
" Greatest mean daily press.,	30 553	30·509	30·611
" " date, . .	Jan. 18.	Nov. 16.	Dec. 18, 1856.
" Lowest—Inches, . .	29·127	29·216	28 884
" " date, . .	Jan. 16.	Feb. 24.	Ap. 21, 1852.
" Least mean daily press.,	29·298	29·390	28·959
" " date, . .	Jan. 16.	March 16.	Ap. 21, 1852.
" Mean daily range, .	0·157	0·160	0·156
" Means at 7 A. M., .	29·879	29·863	29·889
" " 2 P. M., .	29·835	29·822	29·849
" " 9 P. M., .	29·877	29·852	29·875
" " for the year,	29·864	29·846	29·871
Force of Vapor—Greatest—Inches,	0·980 in.	0·939 in.	1·059 in.
" " " date, .	Aug. 10.	Aug. 8.	June 30, 1855
" " Least—Inches, .	·027	·040	·013
" " " date, .	Feb. 4.	Dec. 20.	Feb. 6, 1855.
" " Means at 7 A. M.,	·316	·308	·324
" " " 2 P. M.,	·322	·316	·339
" " " 9 P. M.,	·332	·330	·344
" " " for the year,	·323	·318	·336
Relative Humidity—Greatest—per ct.,	100 per ct.	100 per ct.	100 per ct
" " " date,	Jan. 21.	Often.	Often.
" " Least—per ct.,	15·0	18·0	13·0
" " " date, .	April 26.	Ap. 27; May 8.	Ap. 13, 1852.
" " Means at 7 A.M.,	74·5	74·9	76·0
" " " 2 P.M.,	56·7	56·4	57·4
" " " 9 P.M.,	70·5	71·5	72·2
" " " for the year,	67·2	67·6	68·5
Clouds—Number of clear days,* .	93	100	110
" " cloudy days, .	272	265	255
" Means of sky cov'd at 7 A.M.,	63·4 per ct.	62·9 per ct.	59·8 per ct.
" " " " 2 P.M.,	63·9	62·8	60·3
" " " " 9 P.M.,	49·9	52·5	45·2
" " " for the year,	59·1	59·4	55·1
Rain and melted Snow—Amount, .	49·642 in.	45·656 in.	45·328 in.
No. of days on which Rain or Snow fell,	143	134	128
Prevailing Winds—Times in 1000,	N65°19′W·149	N58°40′W·159	N73°55′W·205

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* Less than one-third covered at the hours of observation.

A General Abstract of the Meteorological Observations made at Philadelphia during the year 1863.—By Prof. James A. Kirkpatrick.

Latitude 39° 57½′ N. Longitude, 75° 10⅓′ W. from Greenwich. Height of Barometer fount, sixty feet above mean tide in the Delaware River.

1863.	Thermometer.									Barometer.								Dew Point.			
			Range.			Means.						Range.		Means.				Means.			
Months.	Maximum.	Minimum.	Monthly.	Mean daily.	Mean daily oscillation.	7 A. M.	2 P. M.	9 P. M.	Average.	Highest.	Lowest.	Monthly	Mean daily.	7 A. M.	2 P. M.	9 P. M.	Average.	7 A. M.	2 P. M.	9 P. M.	Average.
	°	°	°	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	°	°	°	°
January,	61	14	47	5·60	12·90	33·95	40 95	36 64	37·18	30·571	29·127	1 444	·266	29·925	29·867	29·905	29·899	27·80	29 68	29·24	28·90
February,	54	5	49	7·54	13·57	30·46	37·70	34·04	34·07	30·671	29 345	1·326	·265	30·031	29·977	30·024	30·011	24 10	25·33	25 09	24·84
March,	63	15	48	6·47	13 81	32·29	40·94	35·82	36·35	30·384	29·422	·962	·214	29·882	29 829	29·880	29·864	25 38	25·54	26 83	25 92
April,	71	30	41	5·41	14·90	43·95	54·35	47·40	48·57	30·185	29·260	·925	·164	29·788	29 752	29·808	29 782	33·55	33 94	36·72	34·74
May,	90	40	50	5·43	19·15	59·32	71·76	62·64	64 57	29·975	29·295	·680	·0 5	29·783	29·736	29·769	29·763	50·00	49·10	52·19	50·43
June,	91½	52	39½	5 18	16·02	65·78	74·75	67·90	69 48	29·994	29·321	·673	083	29·743	29·712	29·756	29 737	54·22	53 94	55·13	54·43
July,	88	65	23	3 22	10 81	74·40	80·24	76·32	76·99	29 98	29 524	·401	·079	29·800	29·780	29·807	29 796	66 83	66·71	67·44	67 09
August,	95	58	37	3·79	16·00	74·95	85·50	78·05	79·50	30·119	29 699	·420	·100	29 878	29·850	29 868	29·865	66·00	63·00	65 00	64·67
September,	83	41	42	4·22	14·65	61·57	70·68	64·70	65·65	30·31	29 281	1·031	·116	29·940	29 896	29 939	29·925	52·51	54·32	54·47	53·77
October,	74	32	42	4·93	16·10	49·79	61·89	54·1[illegible]	55·29	30 245	29·557	·688	·168	29·920	29 885	29 925	29·910	43·29	43 54	44·93	43·92
November,	69	25	44	6·35	14·37	42·45	52·63	46 12	47·07	30·249	29·434	·815	·185	29·863	29 804	29·854	29 840	34·39	34·16	35·00	34·52
December,	60	15	45	6·54	13 26	30 87	39·13	34·68	34 89	30·495	29·167	1·328	·223	29·999	29·935	29·981	29 972	23·59	25·74	25·10	24·81
Annual means,	95	5	90	5·39	14·63	49·98	59·21	53·21	54·13	30 671	29·127	1·544	·157	29·879	29·835	29 877	29·864	41·81	42·08	43·10	42 33
Winter,	64	5	59	6·53	13·07	32·22	39·47	35·13	35·61	30 671	29·127	1 544	·243	29·963	29·913	29 955	29·944	26·12	27·43	27 29	26·9
Spring,	90	15	75	5·77	15 95	45·19	55 68	48·62	49·83	30·384	29 260	1·124	·154	29·818	29·772	29 819	29·803	36·31	36 19	38·58	37·0
Summer,	95	52	43	4·06	14·28	71·71	80·16	74 09	75 32	30·119	29 321	·798	·087	29 807	29 781	29 810	29 799	62·35	61·22	62 62	62·0
Autumn,	83	25	58	5 17	15·04	51·27	61·73	55·00	56·00	30·312	29·281	1 031	·136	29 908	29 862	29·906	29 892	43·40	44 01	44 80	44·07
Means for 12 years,	100½	—5½	106	5·55	15·11	49·71	59·90	53·14	54·25	30·704	28 884	1·820	·156	29·889	29·849	29·875	29·871		43·40		

TABLE (*Continued*).—*A General Abstract of the Meteorological Observations made at Philadelphia during the year* 1863.

1863.	Relative Humidity.							Force of Vapor.							Clouds. Sky Covered.				Rain or melted snow		Winds.	
				Means.							Means.				Means.							
MONTHS.	Maximum.	Minimum.	Range.	7 A. M.	2 P. M.	9 P. M.	Average.	Maximum.	Minimum.	Range.	7 A. M.	2 P. M.	9 P. M.	Average.	7 A. M.	2 P. M.	9 P. M.	Average.	Amount.	No. of days it fell.	Monthly resultant.	No. of times in 1000.
	Pr. ct.	Pr. ct.	Pr. ct.	Per cent.	Per cent.	Per cent	Per cent	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Pr. ct.	Pr. ct.	Pr. ct.	Pr. ct.	Inch.	Days	Direct'n from	
January,	100	38	62	78·0	65·3	74.8	72·7	·462	·057	·405	·166	·175	·172	·171	68·1	70·0	60·6	66·2	4·698	14	N 50° 26′ W	175
February,	100	29	71	77·3	63·5	70·9	70·5	·322	·027	·295	·142	·149	·146	·146	73·6	62·9	63·9	66·8	3·824	13	N 56 19 W	139
March,	94	26	68	75·4	56·4	70·2	67·4	·445	·050	·395	·147	·151	·153	·150	68·1	72·3	55·2	65·2	6·379	16	N 37 45 W	235
April,	96	15	81	68·8	50·4	68·2	62·5	·404	·072	·332	·207	·213	·232	·217	57·0	75·3	58·0	63·4	7·294	16	N 30 8 W	223
May,	91	28	63	72·3	48·3	69·8	63·5	·700	·189	·511	·375	·365	·406	·382	51·0	55·5	38·1	48·2	4·792	11	N 77 0 W	77
June,	94	22	72	67·9	51·0	65·6	61·5	·687	·207	·480	·436	·433	·446	·438	69·7	66·7	46·3	60·9	4·053	11	N 61 23 W	77
July,	92	43	49	77·7	64·6	75·2	72·5	·812	·399	·413	·663	·660	·684	·669	90·0	80·3	50·6	73·7	5·690	19	S 10 53 W	157
August,	88	41	47	76·8	60·0	75·0	70·6	·980	·268	·712	·585	·590	·610	·595	55·0	60·0	45·0	53·3	1·440	9	N 84 17 W	175
September,	90	38	52	72·6	57·6	69·8	66·6	·784	·166	·618	·423	·452	·449	·441	54·3	53·7	40·7	49·6	0·978	7	N 47 7 W	115
October,	95	28	67	78·9	51·5	71·7	67·4	·648	·094	·554	·303	·306	·320	·310	60·3	59·0	40·6	53·3	2·663	10	N 61 23 W	153
November,	97	25	72	73·7	52·2	66·0	64·0	·463	·074	·389	·208	·214	·216	·213	57·3	54·3	52·3	54·6	2·960	7	N 82 46 W	371
December,	96	35	61	74·1	59·8	68·5	67·5	·486	·051	·435	·142	·156	·145	·148	56·1	56·8	47·4	53·4	4·871	10	N 72 0 W	243
Annual means,'	100	15	85	74·5	56·7	70·5	67·2	·980	·027	·953	·316	·322	·332	·323	63·4	63·9	49·9	59·1	49·642	143	N 65 19 W	149
Winter,	100	29	71	77·9	63·4	73·5	71·6	·462	·027	·435	·154	·162	·160	·159	64·7	63·9	55·7	61·4	10·077	35	N 68 38 W	199
Spring,	96	15	81	72·3	51·7	69·4	64·5	·700	·050	·650	·243	·243	·264	·250	58·7	67·7	50·4	58·9	18·465	43	N 40 15 W	170
Summer,	94	22	72	74·1	58·5	71·9	68·2	·980	·207	·773	·561	·561	·580	·567	71·6	69·0	47·3	62·6	11·183	39	S 63 26 W	80
Autumn,	97	25	72	75·1	53·8	69·2	66·0	·784	·074	·710	·311	·324	·328	·321	57·3	55·6	44·5	52·5	6·601	24	N 75 50 W	201
Means for 12 years,	100	13	87	76·0	57·4	72·2	68·5	1·059	·013	1·046	·324	·339	·344	·336	59·8	60·3	45·2	55·1	45·328	128	N 73 55 W	205

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CIVIL ENGINEERING.

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On the Construction of Wrought Iron Lattice Girders.

By Thomas Cargill, C. E.

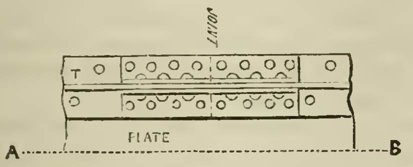
(Continued from page 21.)

In the examples given of the flanges of double lattice girders, the connecting medium between the tension and compression bars composing the web, and the plates which are a portion of the flanges, has been supposed to consist of two pairs of angle-irons, riveted longitudinally to the plates throughout the whole length of the girder. In some instances tee-irons are employed as the means of attachment, each pair of longitudinal angle-irons being replaced by one tee-iron. The table of the tee-iron is riveted to the plates in the ordinary manner, and the bars of the web are attached to the tongue or rib, on the outside and inside successively. This substitution is well adapted for girders of small spans, but cannot be used with equal advantage in those of large dimensions. The riveting of the tee-iron through its table portion to the plates is better and more efficient than that of two angle-irons of the same sectional area. If we suppose the centres of the rivets to be the centres of resistance, the resultant of the forces in the former case will pass through the centre of gravity of the tee-iron, and the leverage will be reduced to a mere nothing. On the other hand, considering each angle-iron in the same pair to act independently of the other, the leverage will be equal to $\frac{l}{2}$, taking l to represent the length of the horizontal side of the angle-iron. To a certain extent each pair of angle-irons does act as one, and as the points where the small space left between them for the insertion of the web is filled up by the bars and distance pieces they may be regarded in the light of one large tee-iron.

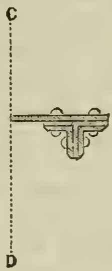
The chief advantage of the pair of angle-irons is in their vertical sides, which can be made as strong and stronger, if necessary, than those placed horizontally. This is not the case with a tee-iron; its rib is its weak part, and in the sections usually rolled is never equal in area to that of the table part. The bars of the web never obtain in their attachment to the tee-iron the same hold which is afforded to them by being riveted in between a pair of angle-irons. Moreover for girders of large span, the necessary amount of sectional area to be given to a pair of angle-irons joining the web to the flanges could not be obtained in one tee-iron, unless specially rolled for the purpose; and there are no commensurate advantages attending its use, to warrant the extra expense which would be thus incurred. In small girders, particularly in those which have a single lattice web, and in others where the sectional area of the vertical sides of the flanges is not required to be very large, such as for instance in the cross girders of a bridge, it will be found simpler to use a single tee-iron instead of double angle-irons. The amount of riveting will be exactly the same in both arrangements. The tee-iron affords somewhat greater facilities with respect to the workmanship, and there is a saving of filling pieces effected in consequence of the bars of the web being attached directly to the tongue of the tee-iron on either side. The only packing pieces required will be at the crossings of the bars of the web; and their thickness will be constant and equal to that of the rib of the tee-iron instead of varying inversely as the thickness of the web, which is what occurs where double angle-irons are used.

In the last number an illustration was given of the joint of one of the double angle-irons forming a portion of the flange of a double lattice girder. Fig. 1 and Fig. 1*a* represent in plan and section respectively the joint of a tee-iron in a similar position. The covering pieces consist of the main plate of the flange, which acts as one wrapper over the table of the tee-iron, and two pieces of angle-iron, which serve as the other wrappers to both the table and rib, and complete the joint. In order that the plate should act as an efficient wrapper its thickness should at least equal that of the table part of the tee-iron. It may be greater, but should never be less, as it would be bad workmanship to rivet heavy tee or angle irons to light plates. This remark does not apply to the riveting of upright angle or tee irons to the sides of plate or box girders, but it must be borne in mind that in the latter instance the upright irons are in comparatively very short lengths, and only intended to act as stiffening pieces to the sides, their sectional area forming no item in the calculations of the strength of the girder. But even in such examples there is a proper limit to the weight of the upright stiffening irons, as may be inferred from what has been already said respecting the riveting together of very unequal thicknesses of iron.

Fig. 1.



Fig, 1*a*.



It will be necessary in the joint we are now considering that the section of each angle-iron wrapper should equal that of the tee-iron to be jointed. It will be seen on referring to the section in Fig. 1*a* that each angle-iron only covers part of the table of the tee-iron, and has nothing to do with the other half which is covered by the other wrapper. Let A = sectional area of the whole tee-iron and a that of the table; since both the angle-iron wrappers will be of the same dimensions, put A_1 for the sectional area of either, then $A_1 = \left(\frac{2A-a}{2}\right)$. In the ordinary sections of tee-iron the rib is rolled of an uniform thickness with the table, although if necessary it can be made either greater or less. Considering the thickness uniform throughout; putting l for the length of the table, l_1 for that of the tongue or rib, and t for the thickness, we have $A = (l+l_1)t$ and $a = lt$; substituting these values for A and a, we find $A_1 = \frac{(l+2l_1)t}{2}$. Let l_2 and l_3 equal the length of the two sides of the angle-irons measured from out to out, and put t_1 for the common thickness, which it is necessary to find before the size of the angle-iron can be determined upon. We shall have, using this notation, $A_1 = (l_2+l_3-t_1)t_1$, and our equation will be

$$(l_2+l_3-t_1)t_1 = \frac{(l+2l_1)t}{2}.$$

Solving for t_1 we obtain

$$t_1 = \pm\sqrt{-\left(\frac{l+2l_1}{2}\right)t+\left(\frac{l_2+l_3}{2}\right)^2}+\left(\frac{l_2+l_3}{2}\right).$$

From an inspection of the section, $l_2 = \frac{l-t}{2}$, and $l_3 = l_1$.

Let us take *e.g.* a tee-iron of the following dimensions, 5″×4″×½″, that is, let $l=5''$, $l_1=4,''$ and $t=5''$, l_2 will $=2{\cdot}5$, and t_1 will have a value of ·575″, or about $\frac{9}{16}$″, which will give the required section of metal for the angle-iron wrappers. In some situations, the angle-iron wrappers might be dispensed with, as a piece of plain bar iron riveted to each side of the tongue, and another piece over the table, would be sufficient to secure the joint, the under side of the table being thus left uncovered. As the same number of rivet-holes would have to be punched, and the saving of material in the wrapper would be very trifling, it would be preferable in all cases to employ the arrangement shown in the figure. For bridges designed to carry only a single line of rails, a very light yet at the same time a very strong cross girder may be constructed of two tee-irons forming the upper and lower flanges, jointed together by a lattice web; an ample degree of stiffness will be obtained by using light angle-irons for the struts.

Our iron manufacture has at present arrived at so high a degree of excellence, and the various processes through which plates, bars, and other forms of iron are put, before being fit for girder work, are conducted on so extensive a scale, that the sections of iron required for the web of a lattice girder can generally be obtained rolled in one length. That such was not the case some years ago is evident from the fact that, in the Boyne Viaduct, the bars and angle-irons composing the web are not all in one length, except some of the short ones near the ends of the girders. With these exceptions, the length of the lattice bars is between 33 and 34 feet, far beyond what would be the dimensions in any moderate size girder. It is very doubtful even at present whether it would be advisable to employ rolled sections of iron in such long lengths, although they could certainly be turned out from the bloom in lengths twice as great as the above. There is not the same difficulty in getting long lengths of bar and other sections of iron rolled homogeneously as there is in long plates, nor have the former so much tendency to split into laminæ, or peel under a severe strain, as the latter. The conformation of the fibres, and the mode of working the two kinds of iron, is the chief cause of this difference.

In Fig. 2*a* and Fig. 2 are shown the section and isometric elevation of the joint and wrappers of an angle-iron in the position they would occupy if used in the web of a lattice girder. Both the inner and outer wrappers consist of pieces of angle-iron, one fitting in inside the main angle-irons, and the other covering the joint on the outside. The sectional area of the wrappers will each equal that of one of the main angle-irons, consequently as they will not both be of the same length, the inner one will have a greater and the outer a less thickness than the angle-irons to be jointed. A general formula for finding the thickness of an angle-iron wrapper was given in last number, so that its sectional area should equal that of the main angle-irons. Supposing however, as should always be the case, that the edges of the wrappers are

Fig. 2*a*

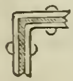
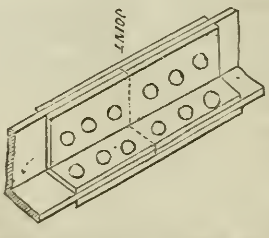


Fig. 2.



flush with those of the angle-irons, as in Figs. 2 and 2*a*, the same result may be arrived at in a much simpler manner. Take for instance the inner wrapper in Fig. 2*a*; it will be sufficient to consider one of the sides, as the same thickness will hold for both. Let l=length of horizontal side of the main angle-iron, and t its thickness, l_1 the length of the same side of the wrapper, and t_1 the thickness required, which it will be seen at once, will be greater than t. We thus have $l\times t=l_1\times t_1$, l and l_1 being both measured from out to out; $l_1=l-t$, and consequently $t_1=\frac{l\times t}{l-t}$. The formula for the thickness of the outer wrapper is not quite so simple. Put l_2 for the length of its horizontal side and t_2 for its thickness; then as before $lt=l_2\times t_2$; but $l_2=l+t_2$ and $lt=(l+t_2\times t_2)$, from which we obtain $t_2=\pm\frac{\sqrt{(4lt+l^2)}-l.}{2}$ In jointing angle-irons, instead of using two pieces of the same description of section for wrappers, four pieces of plain bar iron are often employed instead, but it is evident this does not make so firm and compact a joint as the arrangement shown in the figures. Were it not convenient to procure sections of angle-iron suitable for the wrappers, they might be replaced by two pieces of ordinary plate iron, bent to fit inside and outside the main angle-irons as nearly as they could be made to do. It would not be difficult to accomplish this, for the length of the wrappers for joints in sections similar to the above is generally very small compared to that of the wrappers required for joints in other portions of the girder.

We shall consider the joints in the web of which we are now treating to be in compression, as those in tension are simply joints in a plain bar section, which have been already referred to. Let N equal number of rivets required for each side of a joint in an angle-iron acting as a strut in the web of a lattice girder, l and l_1 the length of the two sides of the angle-iron measured from out to out, and the rest of the notation as before, then

$$\mathrm{N}\times\frac{\pi dt}{2}=\Big([l+(l_1-t)-nd]\Big)t, \text{ from which}$$

$$\mathrm{N}=2\left(\frac{l+(l_1-t)}{\pi d}-\frac{u}{\pi}\right).$$

Except in very long girders the value of n will not exceed 1, and it will depend altogether upon the amount of the variable or moving load, whether it may not be taken equal to zero. If the maximum moving load be not sufficiently great to cause the strut to undergo any severe tensile strain, n may be taken $=0$, on the assumption that bars and plates in compression are not affected by the insertion of rivets completely filling up the holes. As there would be in the struts so very little difference between the net and gross area, and bearing in mind the nature of the strain they are under, we may consider $n=0$, and therefore find $\mathrm{N}=2\left(\frac{l+(l_1-t}{\pi d}\right)\cdot$ This value for N gives the whole number of rivets required in both sides or webs of the angle-iron on one side of the joint, half of them being inserted in the horizontal and the other half in the vertical side. In order to render this arrangement of the joint rigidly correct, it is necessary that two conditions be fulfilled on the part of the angle-irons to be jointed: either the thickness of both the horizontal and vertical sides must be uniform, and their length nearly the same, or their difference in length compensated for by a difference in thickness; in other words, $l \times t$ should nearly equal $l_1 - t)t$ or $l \times t$ nearly equal $(l_1 - t)t_1$. It is evident that were this not the case, and that the two sides of the same angle-iron were very dissimilar in sectional area, we should be compelled to design the joint for each side separately. One side would thus require more rivets than the other, and a longer wrapper. The practical inconvenience would then arise of having to cut off a portion of one of the flanges of the wrapper covering the smaller side of the angle-iron, or make the whole cover longer than necessary, and so weaken the joint. The same necessity for an unequal length of sides in the angle-irons forming the compression bars in the web of the girder does not exist as in the longitudinal angle-irons composing a part of the flanges, and in the latter the disparity of size may be kept practically within such limits as to obviate the contingency alluded to. It should always be remembered that it is safer to trust the rivets of a joint with any excess of strain, than the plates or bars, the former being of superior iron, and also better calculated from their position to resist any additional strain than the latter.

Fig. 3*a*.

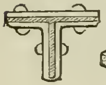
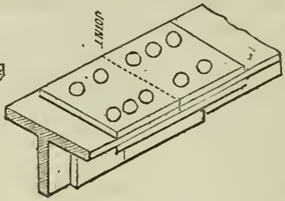


Fig. 3.



A joint of tee-iron is shown in Figs. 3 and 3*a*, consisting of a plate over the outside of the table part, and two angle-iron wrappers on each side of the tongue. This arrangement is nearly identical with that represented in Figs. 1 and 1*a*, the small plate wrapper replacing that portion of the main plates which formed the outside wrapper. The gross section of the small plate will equal that of the table of the tee-iron $= l \times t$. The formula for finding the thickness of the angle-iron wrappers, and consequently their section, will be the same as that given above when the tee-iron was considered as composing part of the flange of the girder. Wherever tee-irons are used of dimensions so small that the tongue portion is less than $2\frac{1}{4}$ inches, it is very much weakened by the insertion of rivets. If such a tee-iron forms one of the compression bars of the web of a girder, it would be preferable to dispense with any wrappers over the joint of the tongue, employing only the plate wrapper over the outside of the table, as seen in Figs. 3 and 3*a*, and two similar smaller wrappers in the inside, which would replace in fact the horizontal sides of the angle-irons shown above. The section of each small plate will equal $\frac{l \times t}{2}$. The tongue is the weak part of a tee-iron, and is intended more to serve as a strengthncing and stiffening adjunct to the table part, than to bear any great strain itself. It was mentioned, when treating of the forms of iron suitable for the bars of the web of a lattice girder, that rivets should not be inserted in the ribs of tee-iron unless either actually unavoidable, or where the ribs were of very large size. In jointing a tee-iron in which the table and the tongue have not a common thickness, it will be necessary, supposing the description of joint to be similar to that given in Figs. 3 and 3*a*, if the difference be considerable, to use angle wrappers in which the thickness of the horizontal side is not the same as that of the vertical. Put, as before, $l \times t$ for the length and thickness of the table part of the tee-iron, l_1 and t_1 for the similar dimensions of the tongue, measured inside; let l_2, t_2, l_3, t_3 be the lengths and thicknesses respectively of the horizontal and vertical sides of the angle-iron wrappers. For the horizontal side of the wrapper we have $l_2 \times t_2 = \frac{l}{2} \times t$. Substituting for l_2 its value $\frac{l - t_1}{2}$ we find that $t_2 = \frac{l \times t}{(l - l_1)}$. If l_3 be measured inside, instead of from out to out, we obtain $l_3 \times t_3 = (l_1 - t_2)\, t_1$; but $l_3 = l_1 - t_2$, and consequently $t_3 = t_1$. It will be sufficiently accurate in practice to make, in the majority of instances, the thickness of either side of the angle-iron wrapper equal to that of the part of tee-iron which it covers. With respect to the length of each side of the angle-iron wrapper, a great deal will depend upon the relative proportion of the tongue and the table of the tee-iron It is manifest that the joint of a tee-iron is open to the same practical inconvenience on this head as that of an angle-iron. In most sections of tee-iron, however, it will be found that $l_1 \times t_1$ nearly equals $\frac{l \times t}{2}$, and therefore there will be no necessity for cutting one side of the wrapper shorter than the other, or weakening by superfluous rivet-holes one portion of the tee-iron at the expense of the other. An angle-iron wrapper, in reality, acts the parts of two wrappers in one piece, and not only covers each portion of the joint belonging to its respective sides, but forms a strong attachment between those portions themselves. It will be seen on referring to Fig. 3*a*, that the angle-iron wrappers evidently strengthen the union between the tongue and the tables of the tee-iron, besides protecting each separate part of the joint, thus causing the whole joint to act together as one piece, in a manner which could not be attained were the two sides of the angle-irons replaced by separate pieces of plate.

A method of jointing a channel iron is represented in Figs. 4 and 4*a*, and consists of covering the joint internally and externally with a couple of wrappers of the same section. The areas will be the same, and consequently their thicknesses will vary, which must be determined before the areas of the different parts, viz: the ribs and channel portions, can be apportioned. Let $l =$ the length on the outside of the

Fig. 4a.

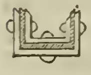
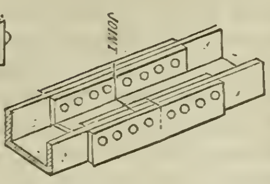


Fig. 4.



channel irons to be jointed, l_1 the length of the sides or ribs measured on the outside, and t the thickness which will be considered uniform throughout. Let us take the inside wrapper first, put l_2, l_3, and t_1 for its dimensions, measured similarly as above, and we have the equation $(l_2 + 2l_3)t_1 = (l+2l_1)t$. It will be seen on reference to Fig. 4*a*, that $l_2 = (l-2t)$ and $l_3 = (l_1 - t_1)$; substituting these values, we obtain

$$[(l-2t+2(l_1-t_1)]t_1 = (l+2l_1)t \text{ and } (l-2t+2l_1)t_1 - 2t_1^2 = -\frac{(l+2l_1)t_2}{2}.$$

Reducing and solving for t_1 we find

$$t_1 = \pm\sqrt{\left[\left(\frac{l-2t+2l_1}{4}\right)^2 - \left(\frac{l+2l_1}{2}\right)t\right]} + \frac{(l-2t+2l_1)}{4}.$$

If we make our main channel irons 6″ × 2″ × ½″, we shall find that t_1 = practically ⅝″, which will give an equivalent section for the inside wrapper; for the outside wrapper, putting l_4, l_5, and t_2 similarly as for the other channel irons, we shall have $(l_4 + 2l_5)t_2 = (l+2l_1)t$. Make the area of the main channel irons, or $(l+2l_1)t = \text{A}$, $l_4 = l+2t_3$, and $l_5 = l_1 + t$, and finally we obtain for the thickness required

$$t_2 = \pm\sqrt{\left[\left(\frac{l+2l_1+2t}{4}\right)^2 + \frac{\text{A}}{2}\right) - \left(\frac{\text{A}}{4t} + \frac{t}{2}\right]}.$$

In this instance t_2 will nearly equal $\frac{7}{16}$″.

In the figures the channel irons are shown sufficiently large to be able to take rivets in their webs or flanges. This advantage could only be obtained in channel irons of very large sections—far too large for the compression bars of the web of any moderate sized girder; and it is questionable whether it would be advisable to insert rivets in the flanges of any channel irons of the sections usually rolled. Partly on account of the above reasons, and partly from the difficulty in practice of obtaining sections of channel iron suitable for the wrappers—for if they do not fit well it would be better to dispense with their use altogether, as they would only make a loose and insecure joint—this description of joint, notwithstanding its great efficiency, would not often be available in girder work. It might be quite safe to insert rivets in the flanges of the main channel irons, which would yet have a very weakening tendency on the inside wrapper, as the space there for the purpose is diminished by its own thickness. It should be remarked that in the description of joint represented in Figs. 4 and 4*a*, the equation for the value of t_1, the thickness of the inside wrapper, will not hold whenever $l_2 \times l_1 < \text{A}$. This is manifest, for at that limit the wrapper is no longer a channel iron, but becomes a thick bar. In small-sized channel irons, the only wrappers which can be applied are an inside and outside piece of bar or plate-iron on the channel part, and the joint then is reduced to that of a simple bar, the flanges being of necessity left uncovered;

nor is the latter alternative of much consequence in the small sections used as struts.

There is another form of iron which may be used for the struts of very large girders, viz: the **H** section. It is represented in section in Fig. 5*a*, which together with Fig. 5 show the details of a method for jointing two lengths of that form of iron. It consists in applying two pieces of channel iron as inside wrappers, and two pieces of bar iron as the outside ones. Let l equal the length of the side of the **H** iron, l_1 its breadth measured inside, and t its thickness uniform throughout. Put A = area of the channel iron wrapper, then $A=\left(2\left(\frac{l+t}{2}\right)+l_1\right)t$. Make l_2 = length of channel part of the wrapper measured from out to out, l_3 = length of ribs on the inside, and t the uniform thickness, then $A=(l_2+2l_3)t_1$; equating these two values of A we have $(l_2+2l_3)t_1=(l+t+l_1)t$, $l_2=l_1$ and $l_3=\frac{l-t-2t_1}{2}$. Substituting these equivalents in the above equations, we have $t_1{}^2-\frac{t_1(l+l_1-t)}{2}=-\frac{(l+l_1+t)t}{2}$, from which the value of t_1 can be easily obtained. The area of the outside wrappers will $=l\times t$.

Fig. 5*a*.

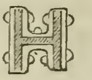
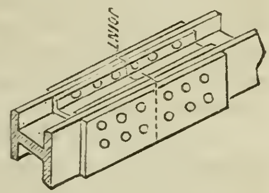


Fig. 5.



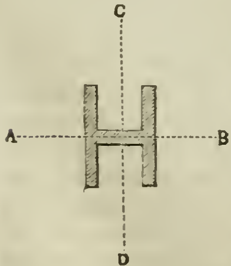
It will be readily seen that this form of iron is not well adapted for struts, either upright or diagonal, when crossed only by single ties, as the riveting at their crossings would have but a very bad hold, in consequence of passing through one side alone of the **H** section. With double ties crossing it on both sides, it would form a very stiff and efficient web. The above difficulty would also be again encountered in attaching it to the flanges. If it were riveted to the outside of the longitudinal angle-irons, it would only hold by one of its sides. To insert it in between the angle-irons, in the usual manner, would necessitate the placing of them much too far apart; the weight of the packing and distance pieces would also be increased to an unwarrantable amount by this arrangement. It might be attached to the outside of the angle-irons, and the disadvantage of the rivets holding only by one side obviated by forging or flattening down the other side and the middle rib so as to cause the rivets to pass through the whole sectional area. Apart from other objections to riveting the bars of the web of a lattice girder to the outside of the longitudinal angle-irons of the flanges, the serious practical difficulty to be overcome in making good sound workmanship in such a case would more than counterbalance any attendant advantage from its employment. With the exception of rolled iron of a plain bar section and light plate iron, all forging, cutting off, or cranking should be avoided as much as possible in girder work.

The iron should, when in its permanent position in the structure, retain the same shape and dimensions as when it issued from between the rollers. No alteration in these respects can be made, even though performed in the best manner by machinery, without doing more or less damage to the material. In a tension bar the resultant of all the strains may be considered as acting along the centre line or longitudinal axis of the bar; if the original shape of the bar be altered in any manner with respect to this axis, an unequal distribution of metal is produced, and the strength is no longer preserved uniform.

An inspection of Fig. 6, will show the facility with which other forms of iron of equal sectional area, may be substituted for the H form. The line CD, divides the section into two tee-irons of equal area. If the same linear dimensions be adhered to, by doubling the thickness we obtain at once a tee-iron of the required section. In any case calling A the area of the H section, we have, making l the length of the table of the tee-iron, l_1 of the tongue, and t the uniform thickness $(l+l_1)\,t = A$. Again, the figure is divided into two equal channel irons by the line AB, and similarly we find, employing our former notation $(l_2+2l_3)\,t_1 = A$. Keeping the lineal dimensions as shown in the figure divided by AB and doubling the thickness, will give a single channel iron of the required area as in the instance of the tee-iron. On examining Fig. 6, it will be perceived that the lines AB, CD, divide the H section into four angle-irons of equal area. For one angle-iron of equal area with the figure we should have $(l+l_1-t)\,t = A$; or, what would amount to the same, if t_1 be the uniform thickness of the H iron, $t_1 \times \frac{1}{4} = t$, and the same linear dimensions may be employed as shown in the figure divided by AB, and CD. This form of iron would be found especially efficient as an upright strut or pillar subject to a compressive force alone, care being taken that the strain was thrown well into the pillar, and its bearings firm and well distributed. Its strength would be greatly impaired, from its peculiar form, by any side or diagonal strain brought upon it.

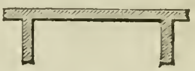
Before concluding our subject for the present, we purpose to make a few remarks representing the net and gross area of a girder—or, what may be used as equivalent terms, but of a more extended range, the net and gross weight; the net weight being understood to mean the actual weight of metal required to sustain the whole load, both live and dead, independent and exclusive of all covering plates, rivets, stiffening, and any other extraneous metal whatever. The gross weight will include the net weight, and all other iron forming part of the girder, but will not include the superstructure or any portion of the roadway. In two similar girders of the same span, the gross weight will bear the same proportion to one another, as the different amount of workmanship, or amount of construction, as it may be termed, bestowed upon each. If in one girder each of the flange plates be half the length of those in the other, there will be double the

Fig. 6.



number of wrappers required, and the gross weight will be proportionately increased. In the Conway Bridge the covers and rivets increase its weight by 31 per cent. For this evil there exist two remedies. One is manifestly the employment of longer plates, so as to diminish the number of wrappers and rivets required for securing the joints: this has been alluded to before and the difficulties to be surmounted mentioned, and it is unnecessary to comment further upon it. The chief obstacle, in so far as the flanges are concerned, is the extra weight or that above what is required by theory, occasioned by the longitudinal riveting of the angle-irons to the plates. Considering the flanges of a girder to consist of two portions, viz: the vertical and horizontal parts, in which the former acts as the connecting medium between the flanges and web, or more properly between the plates and webs, this loss of sectional area and resulting increase of gross weight must always continue until these two parts of the flanges are in one piece. A channel iron would be the simplest form of flange in one piece for the above, but a better section would be that shown below in Fig. 7, in which the only loss of metal would be in the rivet-holes made for the insertion of the rivets which attached the bars of the web to the vertical part of the flanges. Confining our attention to the horizontal part of the flanges, let n = number of rivet-holes punched out per foot run of the girder in the bottom flange, t the thickness of the flange where the holes are made, and w the weight of a cube inch of wrought iron; let x = weight of metal actually required by theory per foot run. Putting d for the diameter of the rivet-hole, the loss of weight equal $ntw\frac{(\pi d^2)}{4}=y$. Accompanying this loss of weight there is also a loss of section, which must be accurately replaced by an increase of metal in the original plate, to keep the weight x constant. Again, as the loss of section or holes for the rivets are filled up by the rivets, their weight must be added, which equals y. The gross weight of the flange per foot run thus becomes equal to $x+y$ instead of x. The increment y is due solely to the necessity of attaching the vertical and horizontal portions of the flanges to one another by continuous longitudinal riveting. To this weight $x+y$ must be added that of the two heads of the rivets, which may be put $=n\times 3d\times w\frac{\pi d^2}{4}=p$.

Fig. 7.



The total gross weight of the horizontal portion of the flange will now equal $x+y+p$. Some idea of the value of p may be gathered from the fact, that in one whole length of tubes in the Britannia Bridge $p=229$ tons. In bridges designed for carrying a line of railway over ordinary public roads of 20 or 25 feet in width, the flanges might undoubtedly be rolled all in one piece, and also all in one length; and thus the necessity for covers and longitudinal riveting altogether done away with, and the net and gross weight nearly equalized. The lattice or open form of web offers many advantages over the plate or solid-sided girder, with respect to the proportion of gross and net weight. In the first place there are very few, and in the majority of instances no joints whatever required in the web of a lattice girder. It has already been remarked that no stiffening irons are needed in the web if properly proportioned, and the correct sections of iron employed for the compression and tension bars. The stiffness necessary should be inherent in the weight of metal allowed for taking the strains to be undergone by each bar, and in the peculiar form and shape given to those bars. Contrast this with the fact, that 21 per cent. of the whole weight of the sides of the tubes of the Britannia Bridge is consumed in stiffening irons and covers. It is well known that the principle of the suspension bridge affords the means of supporting a given load with the least amount of material. Independently of the virtue of the principle on which it is founded, one of the reasons is that in these bridges the net and gross weight more closely approximate to one another than in any other wrought iron construction of bridge. It is not to be supposed that all covers, rivets, and the different connexions, can be ever dispensed with where wrought iron is employed, nor do we advocate a parsimonious distribution of material or the employment of a too rigid and consequently false economy, as is very frequently done; but we maintain that a great deal may yet be accomplished in the reduction of the useless and injurious weight of metal too frequently applied in girder bridges, or in other words, in making them less heavy. The Britannia Bridge at the present day has the merit of possessing the largest single span used for railway purposes; and so long as the ordinary wrought iron is employed, much larger spans will not be reached. With the improvements that are in daily progress in everything concerning the manufacture of steel and a superior class of iron, it is more than probable that at no distant period we shall be able to look for a description of iron or steel or their compounds, which will give twice or three times the strength of our common iron, with half its weight and without any increase in price. At present, did the magnitude of the design or other circumstances warrant the expenditure, we might, with an inferior description of steel, which could be obtained by a less expensive process than the usual one, construct bridges for railway purposes, not on the suspension principle, with spans of from 800 to 1000 feet, with ease and security.

MECHANICS, PHYSICS, AND CHEMISTRY.

For the Journal of the Franklin Institute.

Work, Vis Viva, and Momentum. By De Volson Wood.

Prof. of Civ. Eng. Univ. of Mich.

I published an article upon the subject which forms the title of this paper, in the November number of this *Journal* for 1862, since which, I have received several letters which indicate that I failed to make the subject sufficiently lucid.

As the answers to many of the difficulties suggested, possess a general interest, and will tend still further to elucidate the subject, I have concluded to consider them in the following article.

The difficulties cannot be in the terms used, for they are merely *conventional*; but rather in a correct conception of what they represent in nature. The real office of each expression is best shown by placing them in contrast, or by showing what they represent when applied to the same moving body. This I endeavored to do in my previous article. I will now try to explain some of the difficulties which have presented themselves to others.

One writer says "Work is performed only in giving increased velocity; not in continuing the motion already acquired." This writer either has a wrong conception of *work* or else he has not fully stated the case. Work is overcoming resistance, and resistance may be constantly overcome at a uniform velocity, or even with a diminishing velocity. It may be, however, that the writer had reference to a body free to move; in which case, the work done upon the body is one of inertia; and it is true that no additional *work* is *stored* in the body unless there be an increase of velocity. In this sense the statement is true, but the language does not restrict it to this.

Again, one says "Vis Viva I take to represent a force." Now this is so vague I am inclined to take issue with it and say that *it does not represent a force; but rather what a force has done, or is capable of doing if it acts for a finite time.* To illustrate, suppose a body is allowed to fall freely under the influence of gravity. Now all will admit that the force of gravity near the earth is constant; but after it has fallen for a time its vis viva is MV^2; where M is constant, and V variable, hence it cannot *represent* a constant force.

Again, suppose a locomotive is attached to a train by means of a spring scale, which indicates a constant force of 300,000 lbs. It moves the train from rest with a constantly increasing velocity, until all the surrounding resistances of air, friction, &c., equal the applied force, when the speed will become uniform.

Now observe that the force is constant and the velocity up to a certain speed is variable, how then can MV^2 *represent the force?* If it be said that it represents the force over and above that necessary to overcome the resistances; then I would observe that the resistance of the air increases with the velocity so that the surplus decreases while MV^2 increases. It does not then *represent* either the force applied or the surplus force.

To show what it does represent, let us suppose, to simplify, the case that all the resistances are constant, and that after the train has acquired a velocity V, the locomotive is removed, and the train is permitted to move on until it comes to rest.

Let S be the space over which it moves,
  W the weight of the train,
  f the force for a unit of weight, that is the force, as indicated by a spring scale, which is necessary to draw a unit of weight (say one pound) over the resistances, with a low uniform velocity,

$Wf = P =$ the total force, also equal the total resistances.

Then the total work is

$$WfS = PS.$$

Now if we conceive P to be applied to the train to move it, and conceive all resistances removed so that the train is *perfectly free* to move, it will produce a velocity V in the train at the end if the space S, *i. e.*, it takes the same space for P to produce a velocity V, without resistance that it does to destroy a velocity V, with a constant resistance of P. The law of motion in the former case is the same as for falling bodies, and in the latter that of ascending bodies.

Then since P is constant $\frac{P}{M}$ is the accelerating force and we have

$$V = \sqrt{2\,\frac{P}{M}\,S} = \sqrt{\frac{2Wf}{M}S}$$

$$\therefore \qquad MV^2 = 2PS = 2Wfs.$$

Now observing that P is the true value of the force, we see the second member is twice the force repeated S times, in other words it is twice the sum of all the forces exerted on each unit of the space, S— and the first member must *represent* the same thing.

If the resistances are variable, this becomes general by considering the force on each element of space, and adding them all together, which gives rise to the well known expression $\int Pds$.

Another correspondent says, "A sliding body has a certain vis viva; now if we double the velocity the vis viva will be fourfold; and unless we conclude that dynamical friction increases with the velocity, I do not see how these factors preserve their equality." Dynamical friction does not increase with the velocity; hence we must reconcile the facts on other grounds.

Let φ = the total amount of friction.
$\therefore$ φS = the work on space S,
φS^1 = " " S^1,
V = the velocity at the end of space S,
i = angle of inclination of the plane.

The motion of the body follows the law of falling bodies;

$$\therefore \qquad S = \frac{V^2}{2gS.i.}$$

and for double the velocity

$$S^1 = \frac{(2V^2)}{2gS.i.} = \frac{4V^2}{2gS.i.}$$

$$S^1 = 4S,\ \text{and}\ S^1 = 4\varphi s.$$

So that by doubling the velocity we quadruple the *stored work*. We may observe that the body must pass over four times the space to produce twice the velocity.

Another says, "Gravity does not add equal increments of space in each element of time, and hence doubts whether Wh really expresses the work which gravity does upon a falling body." He admits "that it adds equal increments of velocity in each element of time."

His statements in regard to gravity are correct. Let us apply the latter and see where it will lead us. If a resistance P, be moved with a uniform velocity V, then would the work done in a unit of time be

PV and in a time t, it would be $PVT = PS$ . . (1)

But in a falling body the resistance overcome is the weight, and the velocity is uniformly increasing; so that the work done during any element of time, Δt, is by (1)

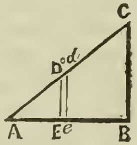
$$WV\Delta t \quad . \quad . \quad . \quad . \qquad (2)$$

But from a law of falling bodies,

$$V = gt \text{ or } \Delta V = g\Delta t,$$

which in (2) gives $\frac{W}{g} V.\Delta V = MV.\Delta V.$

This solved by the calculus gives $\int MV\,dV = \frac{1}{2}MV^2$, but as I wish to make it intelligible to those who do not understand the calculus, I will give the following way of finding the sum of all the products of $V.\Delta V$ between 0 and V. Let any ordinate, as DE, represent the velocity, V. Since the velocity uniformly increases, the line AC will be the limit of the ordinates. Let $Ee = \Delta V$, then will the vanishing trapezoid be ultimately equal the rectangle of which DE is the base and Ee the altitude; hence $DEed = V\Delta V$. The sum of all the infinitesimal rectangles will equal the area ABC. The sum of all the altitudes of the rectangles becomes $AB = V$; hence the area equals $\frac{1}{2}V^2$ and the expression becomes $\frac{1}{2}MV^2$.



My attention has been called to several other points, but they indicate too limited knowledge of the subject, or too little thought bestowed upon it to make a reply to them profitable in this connexion.

Momentum has no reference to the *resistance* to be overcome, but to the *motion* which a body of known mass and velocity will induce in another mass free to move, when the former impinges upon the latter. As the expression indicates, it pertains only to the relations between masses and velocities of different bodies. Momentum may be considered as the measure of an impulsive force; one-half the vis viva, the *sum* of all the energies (or forces) exerted to cause a velocity V. I trust that the relation between momentum and work, in the example used in my previous article, is sufficiently explicit. I will, therefore, drop the subject until circumstances induce a further consideration of it.

For the Journal of the Franklin Institute.

The Forces; Slave Labor; Working Land by Steam; A Cycloidal Cultivator.

Of mundane agencies FORCE—physical energy or power—is unique and supreme. All that we see, and feel, and hear, and do; light and life, forms, qualities, and motions, proceed from it. Its proper use comprises the whole economy of life. It is emphatically *the* talent given to men to profit withal. It places the destiny of every people in their own hands, for the amount at their disposal is not limited, nor are the means of employing it. If it had nothing to do in originating differences between races, it is an imperishable criterion of their varied conditions. While the foremost keep startling the world with wonders wrought by it, others sleep on unconscious of its value. A generic term, it consists of various forces, animate and inanimate; and as the more the latter are brought into play, the less become demands on the former, hence they who add to our working stock of insensible energies, and they who extend their applications to great industrial interests, are, or ought to be, classed among public benefactors. In cheapening force (by increasing its amount) they cheapen its products, and by lessening human drudgery, they help to secure for their species, as respects food and raiment, that condition which is essential to mental and moral culture.

The forces constitute a series that grows with man's growth. They lift him from one stage to another. Taking him up on the lowest and bearing him along towards the highest. We know what the former is, but of the latter we catch only glimpses. With his own strength only man is, and has always been, a savage. The ass and the horse raised him a degree, the ox and the buffalo another. Yet from our standpoint how low appears the status of the Arabs, the Tartars, and others, and with the additional elephant and camel, that of the people of India and of Asia generally. In running and falling water, and subsequently in wind, forces very different from the former were realized. Inanimate and therefore insensible to hard usage, and requiring neither food nor rest, they added vastly to his working capital. What the result was, we have a graphic representation in the oldest of existing people—the Chinese. They, of course, are considered as having acquired only a partial civilization, but we apprehend that it is as complete as those forces could effect. With the same, no people have surpassed them. We doubt if any have equaled them. The next movement was the greatest, and is recent. The agent, as may be supposed, was not, like the foregoing, derived from bodies put in motion and kept moving by nature. The advance of modern society could not have been effected by one, nor by all of them: they prepared the way for the leap, but could not take it. It was the result of the advent of a higher class of agents; such as demand a further stretch of intellect to evolve and control—such as man himself is to call forth for himself from bodies at rest.

Competent (as far as reason can reach) to meet the expanding wants of society for all time, they may be the last as they assuredly are the greatest in the series. Varied and palpable proofs we have of national prosperity and power in the augmentation of their leading representative, *Steam*. Look abroad and see if the first nations are not its greatest consumers. The richest they must needs be, since wealth arises solely from productive labor, and they the chief dealers in it. Close the coal mines of Great Britain, or let the fires of her steam motors die out, and she will descend at once from her high position among the nations. No possible amount of human ingenuity and industry and of animal power, could arrest her declension. No, not the living twelve hundred millions of human beings could do the work which steam is doing for her. And what it is doing is but a fraction of what it is destined to do for her, and for all who choose to employ it. There can be no monopoly of it, no quarreling about it. Free as air, cheap as wood and water, those who have it not have themselves to blame.

[*En passant:* Had the study of nature's laws respecting the Forces she has provided for man, by which to work out his destiny, been included, as it ought to be, in College instruction, this terrible war that is converting whole States into graveyards, might have been postponed—perhaps had not broken out. At all events, if its authors and abettors had been impressed with the fact that there are forces ten times less costly than negro labor, and ten thousand times more abundant and effective—that one of them can do more work in a week than their slave population in a year; that, in short, inanimate powers are ordained to supersede human slavery, and even to ameliorate the labor of animals, they surely would have paused ere calling up the demons of rapine and blood—they would have been reminded that the vast amounts of slave labor already displaced by steam are an earnest of more, and possibly have been disposed to wait for further developments of a power that seems all but omnipotent for human affairs, and only waiting to be applied to them. It is true that great social elements make themselves felt slowly. They must do so if genuine, for nature produces nothing abruptly; but steam has been growing upon us for a century or more, and has now become widely acknowledged as prominent among productive forces.

If there is anything made manifest in the economy of the planet, it is that living forces are weak, irregular, and quickly consumed—-that they are introductory to the powerful, untiring, and exhaustless inorganic energies—that it is by these the work of the world is to be done, and that compared to them the amount of animal labor is a trifle, and that of human toil—a nothing. Slavery, irrespective of the habits it imparts, is purely a question of labor. When it becomes less profitable than other forces at hand, that is the signal for its dismissal. Its upholders may object, but it must yield to the law, and give place to inanimate power: the world is moving and is not to be stayed by them or for them. Hence, secessionists have undertaken something like an impossible problem in attempting, at this day, to raise a new political structure with negro bondage for its basis and corner stone—and this too in immediate contact and competition with free white institutions. The time for it has passed. The world has outgrown it. But such is not their belief. Reared in an atmosphere of slavery, they do not perceive why other people should object to live and move and have their being in it. There have been old hospital patients that preferred the tainted air of the wards to fresh breezes outside.

It is one of the thousand proofs of the support enlightened Christianity receives from the doctrines of science—every one of which *is* divine—that the Creator does not appear in His works in the character of a Moloch on whose shrines negroes are to be perpetually offered up, but in that of a tender and impartial parent, who has made the amplest provision for relieving every race from oppressive labor.

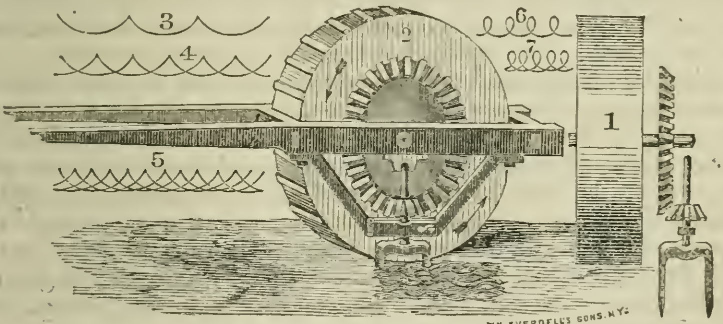
It is vain to wish the South had been willing to resolve the difficulty into a simple question of labor. Perhaps it is equally futile to wish the North would offer a premium for a practical demonstration that slave labor can be replaced by other agents with advantage to the planters. The offer of a single day's cost of the war would be more than sufficient to set the keenest of the world's engineers at work to extend the application of steam to out and in door labor, and to stir up the leading chemists in every country to hasten the birth of forces awaiting their assistance—among them *atmospheric pressure*, the use of which as a popular motor, and the most popular one, seems only held back as by a thread. A cheap device by which to decompose or otherwise promptly to get rid of air or vapor under a piston is all that is wanted. That gained, slavery could not survive it an hour—and it is one of those things that may be hourly looked for.]

The superiority of modern arts is due to the introduction of steam power. It has diminished the cost by increasing the products of industry—it has added to the elegances and multiplied the conveniences and comforts of life, wrought a revolution in almost every department of manufactures, and established a system of traveling by sea and land that would have been deemed miraculous a century ago. It has sown knowledge broadcast by making books common as waste paper. Yet on one great division of labor it has scarcely entered. It has done nothing, or next to nothing, in tilling the land. Not a tithe of it has anywhere been brought under culture. True, an adequate force was long wanting, but one is now sufficiently developed, nor is there room to doubt that steam is ordained to rival in the field what it has accomplished in the factory: that is, to double and quadruple the earth's harvests—to provide, and keep providing, breadstuffs for a population destined, through cycles of ages, to increase in numbers and intelligence. Why has it not been pressed into this service? Are the arts not sufficiently matured properly to apply it? Or is not the delay rather due to attempts to work an old implement with it—one to which it cannot be profitably adapted?

The plough arose from attempts to substitute animal for human labor, and certainly a device better adapted for the purpose, in the simplicity of its form and action, and in the direct application of the power, it would be difficult if not impossible to name. It belongs however to living forces. It came in with them and cannot long survive them. It has been the subject of numerous modifications without affecting its cardinal features, and these it must retain as long as it is associated with draught animals.

We all know that as the arts mature, modes of employing Force become more and more varied. To the extensive adoption of circular for straight motions may be ascribed, in no small degree, the progress they have made in our times; and no one needs reminding that the general effect of steam on transmitting and operating mechanisms has been the substitution of continuous-rotary for alternating-rectilinear movements—a change to which the plough cannot conform without itself being changed. Effectually to cultivate the soil by steam, the working implements must, we apprehend, be revolving ones, and their action, instead of being derived from the forward movement, be independent of it.

Suppose a modern engineer who had never turned his attention to agriculture was employed to bring extensive tracts of wild land into cultivation—to break up the soil and comminute it to the depth of 8 or 12 inches, how would he thus prepare it for seed? I venture to say that he would no more propose any thing analogous to the plough than he would reinvent the flail for thrashing, wait for a wind to separate chaff from wheat, or pound rice into flour in a mortar. Steam would be his power, and to it the form and action of his implements would have to accord. Their movements would be rotary. They would pare away the soil to the requisite depth before them, and leave it thoroughly lightened up behind them. This they would effect, instead of a partial disintegration by dragging a blunt instrument horizontally through it. In brief, I suppose he would develop something more or less allied to the following device.—Though designed to be worked by steam, it is figured as adapted to horse power, on which account the shafts and large drum are introduced---appendages useless when steam is used.



It consists simply of two perpendicular prongs with flattened or sharpened edges—or strong narrow knife blades—formed on the lower end of a short vertical axle, with a pinion which, by its connexion with a bevel wheel, rapidly revolves them. See fig. 1. One side of the bearing piece that supports them is hinged and fastened by a hasp, to allow them to be readily slipped into and out of their place. See fig. 2. Such is the implement, and the whole of it. All the rest is merely to work it. It is little else than a revolving edged fork. The length of the prongs or cutters is of course determined by the depth of soil they are to stir up. As their cost is trifling, three might be kept on hand—a 4-inch, an 8-inch, and a 12-inch one.

In some cases one, but in almost all cases two prongs will be sufficient. If three are required they should be arranged triangularly—that is, at equal distances from the axle. By no possibility can one run into the track of another. The ordinary tracks of a single prong are represented at 3 and 6, of two at 4 and 7, and of three at 5.

The tracks are cycloidal curves, which may be varied from undulating or wave lines to intersecting circles, or figures approaching circles.

The action of the ploughshare is well known to be that of a wedge pressing on the soil beneath it, and with a force equal to that which displaces what is above it.—Every year's ploughing adds to the compression, since every force brought to act on the surface reacts on the undisturbed base. The effect is obvious in old fields. Gravelly and sandy bottoms have become hard and compact almost as stone, and subsoils so dense as to exclude the circulation of air through them. With this cultivator nothing of the kind can occur, since the acting parts transmit not a particle of pressure below. Moreover, neither the feet of the ox or horse, nor the wheels of a steam motor, travel over the ploughed ground. Every thing moves over the unploughed surface except the prongs, and they are suspended from above.

As there is no dragging action there can be no slipping of the wheels of a steam motor—that which has given experimenters most trouble—for the force given to the prongs reacts *within* the carriage—not without. Hence the forward force has only to move them up to their work—not to do it. Their action may be likened to the teeth of circular saws cutting their own way through a log, and only requiring it to be kept up to them. As the prongs act equally in any direction, their motion has only to be reversed at the end of a furrow or field.

I may be wrong, but I suppose any required depth of fair soil may be as completely loosened, lightened up, and prepared for seed, *at one operation*, by this cultivator, as by any one now in use, and with a smaller outlay of force. That which is required to cut 12 inches deep can, I think, be little more than half what is consumed by subsoil ploughs. It is known that thin soils, resting on hard pan, yield greater crops if the pan be loosened to permit the roots to shoot into it. Now these revolving prongs can readily work into it, and without mingling it with the soil, while a succession of roots will gradually fertilize and enrich it.

With the exceptions of breaking up new lands, and working among stones and roots, there seems to be few operations of the old plough which this cultivator cannot perform. It appears to be well adapted to give an intimate and perfect stirring up to light and medium soils; and such is said to be the general character of those of England and France, and of other parts of Europe—also of India, and the sugar and cotton lands of our Southern States.

The hold the cutting prongs take, can, of course, be regulated to slice off portions of any determined thickness—and as they can be arranged that even a man's power could work them in fine soils, possibly market gardeners may find an implement of the kind more economical than their ordinary apparatus.

After citing supposed points in this Cultivator, it would be just to indicate its weak ones. But what they are, practical men must determine. It is not supposed to be free from defects, nor do I think, as already intimated, that it is calculated, in its present form, to supersede the plough, except in fine, light, and loose medium soils. Still, where it cannot act the part of a principal, it may serve as a useful accessory. The question turns on the value of this *cycloidal movement of vertical blades or cutting prongs.* The principle is a novel one in agriculture, and may, at least, suggest an improvement in working the soil, if it be not itself one. E.

New York, Dec. 21, 1863.

On a new Method of Casting Guns Hollow. By Mr. OUBRIDGE.

From Newton's London Journal of Arts, December, 1863.

The author remarked that the subject which he proposed to consider that evening was one upon which much had been already said and written. He believed, nevertheless, that it was not exhausted; for it was a fact that, as yet, no heavy gun had been produced which answered all requirements. Brass guns had had their day, cast iron guns had been long used, were at one period almost totally condemned, but were now cropping up again, and much might be adduced in their favor. Wrought iron, in multifarious forms, had been employed in the manufacture of guns, each form having peculiar advantages; but the results, on the whole, were not satisfactory. Steel had also been introduced for the purpose; but in spite of its great cohesive strength, it had been found impossible hitherto to make from it good, sound, and serviceable guns of large calibre. Many practical difficulties stood in the way of its employment in this direction, although certainly some very successful efforts had been made to construct light and small pieces of ordnance of that material. Compound guns, composed of cast and wrought iron—the one encasing the other—had also been tried, with variable and uncertain effects; but as yet no absolute rule had been deduced for the guidance of those whose duty it was to manufacture heavy guns. Time, and the expenditure of much more of the public money, might effect this great desideratum; but it had not yet been achieved; and it was, therefore, the duty of practical and scientific men to endeavor to solve the problem "how best to manufacture heavy guns?" He (Mr. Oubridge) intended to contribute a few items to the mass of existing information on the subject; and he might state that such knowledge as he had to impart had been gained from his own experience and experiments in the iron foundry. It was required, in the production of large pieces of ordnance, that the material used should be made to offer the fullest possible resistance to the bursting strain to which it would eventually be exposed, and that the cohesive strength of that material should be completely maintained. Perhaps, before advancing his own views, he might be permitted to refer to the method of casting heavy guns, which had been largely practised during the unhappy contest which still raged in America. The name of Dahlgren would no doubt be familiar to his hearers in connexion with the American civil war, and the guns which were known as "Dahlgrens" possessed some distinguishing features. They were cast hollow, the theory of their inventor being that it was desirable "to reduce the neutral axes of his gun as nearly as possible to the centre of the thickness of the metal of which it was formed." This would be clearly seen to be the case from the diagram produced. If, on the contrary, the gun were cast solid, the cohesive power of the metal would be diminished as it approached the centre. By casting the gun hollow, this deteriorating process would be lessened by the pressure of the core in the mould. Dahlgren had evidently well considered the laws of cohesion and disintegration which governed these results. He had also adopted the plan of assisting the cooling and contracting, by pouring a stream of cold air through a tube of iron inserted in the core barrel. In this he was perfectly right, for it ought to be understood that if the cooling process operated entirely from the outer portion of the casting, it was, to use a familiar illustration, like casting an iron ring upon a mandril—an operation which they all knew would be a senseless proceeding. This plan had, however, been persistently followed until the Armstrong gun was introduced. He (Mr. Oubridge) had, several years before, submitted to the Select Committee of the Board of Ordnance, a scheme for casting heavy guns hollow, but the reply of that body was, as usual in such cases, unsatisfactory. At the same period an American gentleman made some attempts to accomplish, in this country, the same thing, but without success. For his purpose six cupolas were erected near the Charlton Pier, on this side of the town of Woolwich, and the experiments there conducted were of a costly character. The theory was not the less a true one; the effect lay in its imperfect realization.

There was no doubt in his own mind that homogeneity would be obtained to a far higher degree in large iron castings if they were made hollow in place of being solid. He had had an opportunity a few months back, indeed, of practically demonstrating the fact. A large hydraulic cylinder, requiring 17 tons of metal to cast it, had to be produced. It was to have a very small hole through the bottom end, where the iron would be 15 inches thick. He saw that the intense heat would inevitably melt the core barrel long before the metal composing the cylinder ceased to be fluid. He therefore assumed it to be a favorable opportunity for putting to the test his cherished theory. The casting was to be produced in the foundry of the Messrs. Simpson, at Pimlico, and Mr. Thompson, the manager for that firm, giving his consent, he determined to cool the core by means of an internal current of cold air. The plan of operation was very simple. A perforated tube was inserted in the core barrel, and then communicated, by means of a pipe and valve, with the blast. As soon as the mould was filled with metal, the valve was opened, and the cold air forced into the tube found an outlet through the perforations, whence it impinged upon the barrel: this latter was also perforated so as to allow of the escape of gases from the interior of the core. The result was a complete success, and the cylinder was as sound and homogeneous as could be desired. The cooling process had thus gone on from the centre, instead of the exterior of the casting. When the operation had, as he considered, lasted sufficiently long to prevent the metal remaining in a fluid state, he caused the blast valve to be shut, and in half an hour the core barrel was found to have melted in one place, leaving an aperture three inches in diameter. The blast was then again turned on, and in less than ten minutes the barrel became black. By this method, which was of the most easy application, it was possible to reduce the temperature of the interior of a casting to almost any extent, and in a very short space of time. He desired to throw his mite into the treasury of human knowledge. It was for others to put it out, if they chose, to interest. Perhaps those who might be called upon to produce heavy castings, whether for the purposes of war, or in fulfilment of the more hallowed behests of peace, would be induced to take up the hints which he had that night ventured to give. It was to be hoped that the practical founders of England, especially, would be permitted to exercise their own judgment as to the selection of the various irons best suited for partial castings, and that in the manner of producing those castings they would not be hampered by mere theorizing or "paper engineers," of whom too many had crept into the noble profession of which he was a humble member. He had a reverence for art and science, and for its practical exemplars, but not even ordinary respect for those who pretended to be their disciples, but were nothing more than pretenders.

After the reading of Mr. Oubridge's paper, which was well illustrated by diagrams, a discussion ensued, and this was followed by a vote of thanks.

Proc. Asso. Foremen Engineers.

Decision of the Board of Examiners in Chief in the matter of the Application of J. S. Mason, *for Letters Patent for Design for a Label.*

Reported by H. Howson, Esq.

U. S. Patent Office, October 16*th*, 1863.

Application (A) of James S. Mason for a Patent for a Design for a Label.

On Appeal to the Examiners in Chief.

To understand the merits of this appeal requires a brief explanation of the design.

It is intended for a label to be placed on boxes of shoe-blacking, and is based upon the well-known representation of a boot so highly polished as to reflect images of the objects around it. Starting with this conception, the artist has depicted a boy holding a boot out to a dog, which is startled by the sight of his own figure in it, while the boot-black in the rear is dancing about in ecstasy at the effect. Simple as this is, two important questions grow out of the application.

In the first place, the applicant has described in his specification, and portrayed in his drawing, a duplicate design resembling the other, except that a fighting cock, irritated at the reflection of his form in the boot, is substituted for the dog. And he claims a patent which shall protect both the original design, and this modification.

His argument is, that the merit of the artist lies not in the idea of representing a boot shining like a mirror, nor in the representation of its effects upon the dog or cock; but in what would usually be termed the subordinate figures in the scene, the boy holding the boot out to produce the effect, and the boot-black delighted with it. There is force in the argument; but admitting its soundness, it does not follow that he may monopolize, not only the delineation of the figures which he regards as really the principal features originated by him, but also several different arrangements of figures, with which he may choose to fill up the scene. If there is no merit in these, or if the applicant does not wish to patent them, he may confine himself in his claim to the other more important parts of the representation.

Or he may, perhaps, claim them in one class, and claim them in combination with one set of figures to fill up the sketch in another. But he cannot claim two sets of figures, and thereby monopolize under one patent, what may be two entirely different designs, each perhaps highly artistic and valuable in itself. The Examiner was correct therefore in rejecting the application as long as the petitioner insisted on retaining both modifications of his design in his specification, and so framing his claim as to cover both.

The applicant proposed to amend his specification by inserting in the claiming clause the words "substantially as shown in the drawings;" so as to protect himself from imitations with slight and unimportant variations. This the Examiner refused to allow, and insisted that "a design admits of no variation; any alteration gives matter for special consideration."

There can be no doubt, however, that a mere evasive variation in the design, would not prevent a counterfeit from being furnished as an infringement. The same principle of law would apply in such a case as in actions upon patents for machines. In both instances the patent will be construed exactly as if the phrase "substantially as described" was in the claiming paragraph. This was decided in the most explicit terms as to patents for designs by the late Judge M'Lean, in Root *vs.* Ball and Davis, 4 M'Lean's R. 177; and, until that case is overruled, the question cannot be considered with propriety, as open to discussion elsewhere. And, since the patent is so construed in one case, as well as in the other, there can be no objection to inserting the corresponding expressions in patents of both kinds, and making them declare what they have been adjudged to mean. The Examiner ought, therefore, to have allowed the amendment.

It must not be understood, however, that such an amendment will have the effect of protecting the applicant's right to both the figures he has described.

That cannot be done by one patent, as we apprehend; though the same things may perhaps be effected, substantially, by patenting the remaining parts of the design. It is for the applicant to determine whether he will adopt that course. But while we consider that he has the right to use the expression, "substantially as described," in order to give the fullest notice of what are really his rights, we hold that he has not the right to embrace both his complete designs under one patent; and that he was, therefore, rightfully rejected for refusing to confine his claim to one of them.

The specification contains a second claim embracing the printed inscription around, and underneath the pictorial design. The applicant has therefore attempted to obtain a patent for a design embracing nothing but printing with ordinary type, and the decision of the primary Examiner refusing a patent, was affirmed by this Board, the 8th May last. The reason for their judgment in that case apply with equal force to the claim before us, and it must be regarded as having been properly denied.

The decision of the primary Examiner is, therefore affirmed on both points.

S. H. HODGE, } *Examiners in Chief.*
J. J. COOMBS, }

On the Direct Correlation of Mechanical and Chemical Forces.

By HENRY CLIFTON SORBY, F.R.S.

From the Proceedings of the Royal Society, No. 56.

Perhaps it may be thought somewhat strange that a geologist should undertake such a subject as the correlation of forces; but the very fact of my being a geologist has led to the investigation of which I now purpose to give a short preliminary account. In studying general chemical and physical geology, and especially in examining the microscopical structure of rocks, I have for a number of years been greatly perplexed with a class of facts which pointed both to a mechanical and to a chemical origin. At first I attributed them either to a mechanical or a chemical action, or to the two combined; but in most cases no satisfactory explanation could be given. At length, however, facts turned up which altogether precluded any supposition not involving direct correlation; for they most clearly indicated that mechanical force had been resolved into chemical action in the same way as, under other circumstances, it may be resolved into heat, electricity, or any other modification of force, as so ably described by Grove in his work "On the Correlation of Physical Forces."

The effect of pressure on the solubility of salts has already been made the subject of speculation and experiment,[*] and a considerable number of facts have been described, showing that pressure will more or less influence such chemical actions as are accompanied by an evolution of gas, so that it may cause a compound to be permanent which otherwise would be decomposed;[†] but the results were for the most part so indefinite and unconnected, or of such a character, that

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[*] Perkins, Ann. de Chim. et de Phys. vol. xxiii. p. 410. Sartorius von Waltershausen, Gottingen Studien, 1857. Bunsen, Ann. der Chem. und Pharm. 1848, vol. lxv. p. 70. Favre, Comptes Rendus, vol. li. p. 1027 Thomson, Proc. Roy. Soc. vol. xi. p. 473 (1861).

[†] Sir James Hall, Trans. Roy. Soc. Edinb. 1812, vol. vi. p. 71. Wohler, Ann. der Chem. und Pharm. vol. xxxiii. p. 125. Babinet, Ann. de Chim. et de Phys. (2) vol. xxxvii. p. 183. Lothar Meyer, Pogg. Ann. vol. civ. p. 189. Beketoff, Comptes Rendus, vol. xlviii. p. 442. Gassiot. Brit. Assoc. Report, 1854, p. 39. Favre, Comptes Rendus, vol. li. p, 1027. Berthelot et Pean de Saint-Gilles, L'Institut, 1862, p. 257. Gmelin's. Handbook of Chemistry, published by the Cavendish Society, vol. ii. p. 293.

Mr. Grove does not allude to the *direct* production of chemical action from mechanical force. That this is, however, extremely probable will be evident to all who have considered the manner in which the various physical forces are correlated; for if mechanical force can be produced by chemical action, why should not the converse be true? In this paper I shall endeavor to show that such is really the fact, and that in some cases the mechanical equivalent of the chemical force may be determined.

In order to obtain the necessary great pressure, I have made use of a modification of the method employed by Bunsen; but instead of filling the tubes at the ordinary temperature of the atmosphere and then gently heating them for several hours, I, in the first instance, filled them at a temperature of 10° or 20° C. lower, so that when finally sealed up they contained considerably more liquid than they could hold without pressure at the ordinary temperature of the atmosphere at the time being; and thus, by its tendency to expand, this liquid and any thing enclosed in the tube were subjected to a very great pressure. By keeping the tubes in various parts of the house, according as the weather varied, I have been able to maintain for several weeks or even months a pressure of, for instance, about 100 atmospheres, as measured by means of a capillary-tube pressure-gauge enclosed within the larger tube. Since in all cases I had a second tube which from first to last was treated precisely like the other, pressure excepted, I have been able to determine the effect produced by the pressure with very considerable accuracy—at all events so as to leave no doubt whatever about the general facts. At the same time I wish it to be understood that the results described below must be looked upon only as approximations to the truth.

I will first call attention to the well-known influence of pressure on the fusing-point of various substances, since it is a connecting link between well-established facts and those I am about to describe. Bunsen[*] and Hopkins[†] have shown that substances which expand when fused have their point of fusion raised by mechanical pressure; that is to say, since mechanical force must be overcome in melting, the tendency to melt must be increased by heat before that opposition can be overcome; and the pressure required to keep them solid at any temperature above their natural point of fusion may be looked upon as the mechanical presentative of the force with which they tend to fuse at that temperature. Professor W. Thomson[‡] has shown that, on the contrary, water, which expands in freezing, has its point of fusion lowered by pressure; that is to say, since mechanical force must be overcome in crystallizing, crystallization will not take place under increased pressure unless the force of crystalline polarity be increased by reducing the temperature. Thus, calculating from his experiments and from the known latent heat of ice, and assuming that no heat is gained or lost by contact with external objects, if we had 1 part of ice and 100 of water at 0° C., and then applied a

[*] Pogg. Ann. 1850, vol. lxxxi. p. 562.

[†] British Association Report, 1854, p. 57.

[‡] Trans. Roy. Soc. Edinb. vol. xvi. p. 575.

pressure of 103 atmospheres, the ice would, as it were, dissolve in the water, the whole would become liquid, and the temperature be reduced to —·792° C.; or, in other terms, at that temperature the tendency to crystallize is exactly counterbalanced by that pressure.

Now I find that similar principles held true with respect to the solubility of salts in water. If, when they dissolve, the total bulk increases, pressure reduces their solubility; whereas if the bulk decreases, pressure makes them more soluble; in other words, solution or crystallization is impeded by pressure according as mechanical force must be overcome in dissolving or in crystallizing.

Various authors have written on the volume with which salts enter into solution;[*] but since the subject before us requires a different class of facts to be taken into account, I shall base my conclusions on my own experiments. The volume with which salts exist when in solution, assuming that of the water to remain unchanged, varies greatly in the case of different salts, and also according to the amount in solution and the temperature. Thus, taking sal-ammoniac as an example, when there are 3 per cent. in solution in the volume is as if it expanded 3·40 per cent. on dissolving; whereas when 25·55 per cent. are in solution, the expansion is 11·36 per cent.; and when nearly concentrated at about 13° C., an additional quantity expands on dissolving 15·78 per cent. In by far the greater number of cases, however, there is a contraction on dissolving, and the amount gradually diminishes for each additional quantity entering into solution, so that the mean result is very different from what occurs when the solution is dilute or nearly saturated. It is this contraction or expansion when a small additional quantity is dissolved in a nearly concentrated solution that must be taken into account in the following calculations.

In determining the influence of pressure on the solution of salts, I found it requisite to adopt somewhat different methods according to the peculiarities of the salts. In some cases I sealed up in a saturated solution portions of the salt in clean, solid crystals, and determined the effect due to pressure from their loss in weight; whereas in other cases I sealed up solutions containing more salt than could be dissolved at the temperature at which the experiments were made, and determined the effect of pressure from the difference in the weight of the crystals deposited; being of course careful to make allowance for any difference in the amount of solution in the tube with pressure and in that without, and to avoid any error that might be produced by a different temperature. In all cases I have had a tube with pressure and another without, treated from first to last in precisely the same manner, and kept exactly the same temperature, so that pressure was the only difference; and usually the effect was so well marked that there was no doubt about the result. In the case of chloride of sodium, solution goes on so slowly, and the mechanical equivalent of the force of crystallization is so great, that if pressure had been applied for only a few hours one might have concluded, with Bunsen, that pressure has no influence on solubility; but, by maintaining it for a week or more, there was no difficulty whatever in perceiving that a solution which was quite saturated without pressure, dissolved more under a pressure of about 100 atmospheres.

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[*] Playfair, Chem. Soc. Quar. Journ. vol. i. p. 139. Michel and Krafft, Ann. de Chim. 2 ser. vol. xli. p. 471. Schiff, Ann. der Chemie, vol. cix. p. 325; vol. cxi. p. 68; vol. cxiii. p. 349. Gerlach, Specifische Gewichte der Sazlosungen, &c., 1859. Tissier, L'Institut, 1859, p. 158; 1860, p. 281. Kremers, Pogg. Ann. vol. lxxxv. pp. 37 and 246; vol. xciv. p. 87; vol. xcv. p. 110; vol. xcvi. p. 39; vol. cv. p. 360; vol. cviii. p. 115. Billet, These de Chimie.

The solubility of a salt in water appears to me to result from a kind of affinity which decreases in force as the amount of salt in solution increases. This affinity is opposed by the crystalline polarity of the salt; and when the two forces are equal, the solution is exactly saturated. As is well known, a change in temperature alters this equilibrium; and, according to my experiments, mechanical pressure relatively increases one or other of these opposing forces, according to the mechanical relations of the salt in dissolving. At all events in the case of chloride of sodium the extra quantity dissolved under pressure varies directly with it for such pressures as glass tubes will resist, in the same manner as, according to Thomson's experiments, the fusing-point of ice is reduced. Thus I found that for a pressure of 49½ atmospheres the extra solubility was ·176 per cent., and for 121 atmospheres ·431, which are almost exactly in the same ratio. Hence, if S be the amount soluble without pressure, under a pressure of p atmospheres, the solubility at the same temperature would be $S + ps$, where the values of S and s are independent, and vary for different temperatures and different salts. Future experiments may perhaps show that this conclusion should be modified; but yet it will be well to adopt it provisionally, in order to compare together the mechanical relations of different salts which otherwise would not be so intelligible.

According to Michel and Krafft* and to Schiff,† sal-ammoniac is the only salt known for certain to occupy more space in solution than when crystallized. Hence, under pressure mechanical force must be overcome in dissolving, and experiment shows that, on this account, the relative force of crystalline polarity is increased and the solubility decreased. This is the reverse of what results from an elevation of the temperature, so that the effect cannot be due to heat generated by the pressure, but must be the direct consequent of pressure. Calculating from an experiment where the pressure was 164 atmospheres, which gave a decreased solubility of 1·045 per cent. of the whole salt in solution, a pressure of 100 atmospheres would cause ·637 per cent. less to be dissolved than is soluble at 20° C. without pressure, and the pressure requisite to reduce the solubility to the extent of 1 per cent. would be 157 atmospheres. Expressing this fact in other words, we may say that a pressure of 157 atmospheres is the mechanical force with which the salt tends to dissolve in a solution containing 1 per cent. less than can dissolve at the same temperature without pressure, because the two forces exactly counterbalance one another. In a still more dilute solution the force would of course be still greater, in accordance with the fact of a greater pressure being necessary to prevent the salt from being dissolved. Supposing then that we had a solution a trifle more dilute than that just named, and in such indefinitely large quantity that a cubic inch of the salt could dissolve in it and yet produce no sensible change in its strength, so that from first to last it might be considered to dissolve under a pressure of 157 atmospheres, and also supposing that it was rigidly enclosed on all sides but one, so that the whole expansion must take place in one direction over an area of one square inch, since on dissolving there is an increase in bulk from 100 to 115·78, the solution of this cubic inch would, as it were, raise 2355 ℔s. through the space of ·1578 inch. This is mechanically the same as 371½ ℔s. raised 1 foot, or, the specific gravity of the salt being 1·53, the same as 171 times the weight of the salt itself raised 1 metre. Since it involves no arbitrary unite but the metre, I shall adopt the last expression as the measure of the total amount of mechanical work done by the solution of salts which expand in dissolving, and which may conversely be looked upon as the measure of the mechanical force rendered latent and, as it were, expanded in the act of crystallization when crystals are deposited. The value of this mechanical equivalent of course varies with the strength of the solution, as already remarked.

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* Ann. de Chim. 3 ser. vol. xli. p. 471.

† Ann. der Chemie, vol. cix. p. 325; vol. cxiii. p. 329.

In the case of salts which occupy less space when dissolved than when solid, pressure, like the increased temperature, causes them to be more soluble; mechanical force is lost when they dissolve, and is, as it were, expended in giving rise to solution. When water thus containing more of a salt than could otherwise be dissolved at the same temperature is just saturated under any given pressure, the amount of pressure represents the force of crystalline polarity tending to cause the salt to be deposited in a crystalline form, but which is exactly counterbalanced by that pressure. I will not give the details for each salt, but subjoin a Table of the results at which I have arrived for such as illustrate particular points of interest, the calculations being all made in accordance with the principles already described. I also give them in the case of water, calculated from Thomson's experiments, assuming that, when ice meets and mixes with water, it may be looked upon as dissolving in it; and, as will be seen, the mechanical force thus deduced is of the same general order of magnitude as that generated by the crystallization of salts.

	I.	II.	III.	IV.	V.
1. Chloride of Sodium, . .	13·57	97	·407	·419	157
2. Sulphate of Copper, . .	4·83	60	1·910	3·183	7
3. Ferridcyanide of Potassium, .	2·51	86	·288	·335	42
4. Sulphate of Potash, . .	31·21	63	1·840	2·914	42
5. Ferrocyanide of Potassium, .	8·90	66	1·640	2·485	20
6. Water, . . .	8·93	. .	. .	·991	106

Nos. 2 and 5 are calculated as hydrated crystals.

Column I. gives the expansion of each salt in crystallizing from a nearly saturated solution of water, the volume in a crystalline state being taken at 100.

Column II. gives the actual pressure in atmospheres in the experiment.

Column III. gives the increased solubility due to the pressure given in column II., the total amount of salt dissolved without pressure being taken at 100.

Column IV. gives the increase in solubility that would be produced by a pressure of 100 atmospheres, as calculated in accordance with the principles already described, the same unite being taken as in column III.

Column V. gives the value of the mechanical work that could be done, or, so to speak, the amount of mechanical force set free when the various substances crystallized from a solution containing 1 per cent. more than would be dissolved without pressure, as measured by the number of times its own weight which any unite of the various salts could rise to the height of 1 metre in the act of crystallization. Conversely, it is the amount of mechanical force which becomes latent in the act of solution; and in the case of a still more supersaturated solution it would be greater, and *vice versâ*, in accordance with the fact of the increased solubility varying with the pressure.

On comparing together the various salts, it will be seen that their properties vary very considerably. Thus, under the same pressure, the extra quantity of sulphate of copper dissolved in nearly ten times that of ferridcyanide of potassium. The mechanical equivalents also vary even more, being (for chloride of sodium) about 22½ times as great as for sulphate of copper. On the contrary, the mechanical equivalents of ferridcyanide of potassium and sulphate of potash are the same; but, under equal pressures, the extra quantity of the latter dissolved is nearly nine times as great, owing to the difference in the amount of expansion in crystallizing. This latter is, however, nearly the same for water and ferrocyanide of potassium, whilst, under the same pressure, the extra quantity of that salt dissolved is 2½ times that of ice, in consequence of the much greater mechanical equivalent of the ice. It appears to me that we may provisionally conclude that the increased solubility due to pressure varies directly with the change of volume, and inversely with the mechanical equivalent of the force of crystalline polarity, so that, if S be the total amount of salt which dissolves without pressure, c be some function of the change in volume in dissolving, and m some function of the mechanical equivalent of the force of crystalline polarity, the solubility, at the same temperature, under a pressure of p atmospheres would be $+ \mathrm{S}\,\frac{p.c}{m}$. If the salt be one that expands on dissolving, c of course is negative, and therefore under pressure the solubility becomes $\mathrm{S} - \frac{p.c}{m}$; that is to say, it is diminished, as proved by experiment with sal-ammoniac. If no change in volume took place, we may, I think, also conclude that pressure would not in any way increase or decrease the solubility of salt. Moreover, since, when a solution is just saturated, the force with which the salt tends to crystallize is equal to that with which it tends to dissolve, their mechanical equivalents must be equal and opposite. Hence we may perhaps conclude that, other circumstances being the same, the mechanical equivalent of a salt like chloride of sodium, which so readily attracts moisture, would be greater than that of one like sulphate of copper, which so readily loses even its water of crystallization ; and thus also the relative influence of equal amounts of pressure would be very different, as is confirmed by experiment in the case of these and some other salts.

The facts I have described, therefore, show that there is a direct correlation between mechanical force and the forces of crystallization and solution. According to some chemists, the latter is an instance of real combination ; but, whatever views be entertained respecting its nature, we cannot, I think, deny that the force represents some modification of chemical affinity, or is at all events most closely allied to it. In comparison with some kinds of affinity, it may indeed be, and probably is, weak ; but, yet as I have shown, it sometimes has a very considerable mechanical equivalent, even when nearly counterbalanced by an opposite force ; and since such pressures as glass tubes will resist have no very great influence on what we may perhaps consider a weak affinity, we cannot expect that any pressure at our command would have much influence on strong affinities. I have, however, succeeded in obtaining some results which apparently show that pressure influences undoubtedly chemical changes taking place slowly, and therefore probably due to weak, or nearly counterbalanced, affinities.

The method adopted in this part of the inquiry was to seal up some solid substance in a solution which gives rise to a slow double decomposition, taking great care to have in the tube with pressure, and in that without, pieces cut so as to be of the same size and form, and a solution of the same character, so that, with the exception of pressure, all the conditions were the same. Possibly I may be so fortunate as to discover some case where the affinity is so weak that pressure may determine whether it go forward or not, of which fact the structure of metamorphic rocks furnishes examples ; but hitherto I have only been able to prove that pressure modifies the *rate* at which chemical action takes place. This branch of the inquiry is, however, beset with many difficulties, for the change in volume produced by double decomposition is small, and its determination involves several complicated questions. The volumes of the solids is easily determined ; but that of the salts in solution is not the same when other salts are present as when they are dissolved in pure water, and varies much according to the strength of the solution and the nature of the salts ; and many points are still so obscure, that I shall only give two cases by way of example.

When a portion of Witherite is enclosed in a tube with a strong solution of protochloride of iron, there is a slow decomposition into chloride of barium, which is dissolved, and carbonate of iron, which remains firmly attached to the Witherite, and would ultmately give rise to an excellent pseudomorph. The best conclusion at which I have been able to arrive is, that there is in the change an increase in volume equal to about 10·7 per cent. of the Witherite altered, so that, under pressure, mechanical force must be overcome. In an experiment where everything went on in a very satisfactory manner, the pressure was maintained for three months at from 80 to 100 atmospheres, and for one month was under 80 atmospheres, so that, on an average, it was about 80 atmospheres; and I found that the amount of chemical change was 21·7 per cent. less than when, all other circumstances having been the same, there had been no pressure; thus clearly showing that pressure had, as it were, diminished the force of chemical affinity. If then one cubic inch had been altered under this pressure, it would have overcome a mechanical force equal to that required to raise 1200 lbs. through the space of ·107 inch, which is equivalent to raising twenty-one times its weight to the height of 1 metre; and under the same circumstances 1·278 cubic inch would have been altered when no such mechanical force had to be overcome. Supposing then that in both cases the total energy at work was the same, but in one was altogether expended in producing a chemical result, and in the other in producing partly a chemical and partly a mechanical effect, we may say that the force which gives rise to the purely chemical change, taking place at a particular rate, is equal to that which gives rise to this chemical effect, taking place at ·783 of that rate, and to a mechanical effect equal to the force required to raise in the same space of time 34·87 times the weight the Witherite altered to the height of 1 metre. Supposing also that the power of chemical force varies at the rate at which it gives rise to a chemical change, in the same manner as the power of a mechanical force varies as the velocity of motion imparted by it, we may perhaps conclude that this mechanical force is equal to ·217 of the chemical force, and that the whole energy of the chemical action under the conditions of the experiment was equal to the mechanical power required to raise in the same period of time 160 times the weight of the Witherite altered to the weight of 1 metre. If these principles are correct, a pressure of more than 370 atmospheres would have entirely counterbalanced the force of chemical affinity, since to produce any chemical change it would then have had to overcome a greater force than it possessed. This is so great a pressure that I fear it will be difficult to prove the deduction by experiment; and until some such case can be found, capable of being verified, these calculations must be considered as little more than suggestions, which future investigations may confirm or disprove.

When calcite is sealed up in a mixed and rather strong solution of chloride of sodium and sulphate of copper, slow double decomposition gives rise to malachite, sulphate of lime, and carbonic acid; and though this is extremely complicated, and it is very difficult to determine what would be the change in volume, yet, so far as I am able to make out, until the solution becomes saturated with sulphate of lime, there is a decrease in volume equal to about 8 per cent. of that of the calcite altered, so that, under pressure, mechanical force is the very reverse of being opposed to the chemical change. Three experiments all indicate the same fact, and in which, on an average, the pressure was about 90 atmospheres for two weeks, show that, as a mean of the whole, the amount of chemical change of 17 per cent. more with the pressure than without; thus proving that pressure had, as it were, increased the force of chemical affinity. Calculating according to the principles described above, we may conclude that a pressure of 530 atmospheres would have caused the action to take place at double the rate, and that therefore the chemical action is equivalent to the expenditure of that amount of mechanical force, being thus generated by it. Arguing then in a manner similar to that already described, but modified to suit the different conditions, if there be a contraction equal to 8 per cent. of the bulk of the calcite, there must be a loss of mechanical force capable of raising 28 times the weight of the calcite altered to the height of 1 metre, in the time required for the chemical change; which amount of mechanical energy, as it were, becomes latent, and is transformed into chemical action, and would again exhibit itself as a mechanical force if, by any means, the chemical affinities could be inverted and everything restored to its original state.

In a like manner, other experiments indicate that in some cases pressure causes a slower, and in others a quicker chemical action, whilst in others it has scarcely any influence whatever; and though, for reasons already explained, I say it with some hesitation, yet bearing in mind what is already known respecting the action of pressure on hydrate of chlorine, hydrated hydrosulphuric acid, and other substances described by the various authors referred to in the notes, I think the facts I have described make it very probable that further research will show that pressure weakens or strengthens chemical affinity according as it acts against or in favor of the change in volume; as if chemical action were directly convertible into mechanical force, or mechanical force into chemical action, in definite equivalents, according to well-defined general laws, without its being necessary that they should be connected by means of heat or electricity. On the present occasion I shall not attempt to consider the various geological and mineralogical facts which appear to me to admit of the application of the principles I have described, for many of them are peculiarities in structure of which neither myself nor any one else has ever given a description, and would therefore demand a preliminary notice. However, I may say that it appears to me that a number of facts connected with metamorphic rocks and the phenomena of slaty cleavage, which, to me at all events, have hitherto been inexplicable, are readily explained if mechanical force be directly correlated to chemical action, and if in some cases the direction in which crystals are formed be more or less related to pressure, in some such way as there is a connexion between their structure and the magnetic force shown by the experiments of Plücker, Faraday, Tyndall, and many other observers.

We may also, I think, explain the origin of the impressions on the limestone pebbles in the "Nagelflue" in Switzerland, about which so much has been written in Germany and France, without a satisfactory reason having been discovered; and the same explanation accounts for the mutual penetration of the fragments of which some limestones are formed, and for the banded structure of some which possess slaty cleavage. The curious teeth-like projections with which one bed of limestone sometimes enters into another, also to a certain extent indicate a chemical action depending on mechanical force; and probably the same may be said of some of the peculiarities of slickensides and mineral veins. It is also possible that a pressure of several hundred atmospheres may facilitate some of the chemical changes involved in the transformation of water and carbonic acid into the organic compounds met with in animals and plants of low organization found at great depths in the ocean, and thus to a certain extent compensate for diminished light. I, however, most willingly admit that very much remains to be learnt before we can say to what extent the principles I have described are applicable; and yet, at the same time, cannot but think that henceforth they must be taken into account in many departments of the chemical and physical geology, and will readily explain a number of facts which otherwise would be very obscure.

On the Measurement of the Chemical Brightness of various portions of the Sun's Disk. By HENRY ENFIELD ROSCOE, B.A., F.R.S.

From the Proceedings of the Royal Society, No. 56.

The author has applied the method of measurement of the chemical action of sunlight, which Professor Bunsen and he described in a memoir presented to the Royal Society in November last,[*] to the measurement of the chemical brightness of various portions of the solar disk; and although the observations which have as yet been made, are only preliminary, yet he thinks that the results obtained are of sufficient interest to warrant his bringing them before the Society.

Secchi has shown[†] that the calorific radiation of the centre of the sun's disk is nearly double that from its borders, and that the equatorial regions are somewhat hotter than the polar, whilst observers have long noticed a great difference in luminosity between the centre and edge of the disk.

For the purpose of obtaining a measurement of the relative chemical brightness of various portions of the solar disk, the image of the sun, of about 4 inches in diameter, obtained by a 3½ inch refractor,[‡] was allowed to fall into a camera placed on the instrument, upon a sheet of standard photographic paper prepared according to the method described in the above-mentioned research. The peculiar property of this standard paper is that it can always be prepared of one and the same degree of sensitiveness, and is perfectly homogeneous. The exposure lasted for from 30 to 120 seconds, the sun's motion being carefully followed by a tangent-screw. After exposure, the shade of tint at several points on the picture was determined by comparison with a graduated photographic strip insolated in the pendulum-photometer, and the chemical intensities corresponding by these shades obtained by reference to the Table given in the memoir above cited. The following numbers give the chemical brightness, thus obtained, at various points on the sun's disk on May 9th, 1863. From these numbers it is seen that the intensity of the chemically active rays at the centre is from three to five times as great as that at the edge of the disk, the chemical rays thus showing a wider variation than the calorific rays exhibited as determined by Secchi. This is doubtless owing to the relatively greater absorption effected by the solar atmosphere on the more refrangible chemical rays.

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[*] Abstract. Proc. Roy. Sec. vol. xii. p. 306; Memoir, Phil. Taans. 1863.

[†] Astron Nachr. Nos. 806, 833.

[‡] Kindly placed at my disposal by S. W. Williamson, Esq., Manchester.

Chemical Brightness of Sun's Disk on May 9th, 1863.

	1. At centre of Sun's Disk.	2. At 15° from the edge of Sun's Disk.			3. At edge of Sun's Disk.		
		N. Pole.	Equator.	S. Pole.	N. Pole.	Equator.	S. Pole
No. 1.	100·0	38·8	48·4	58.1	18·7	30·2	28·2
No. 2·	100·0	52·8	. .	56·6	30·5	. .	41·0

Hence it is likewise seen that on May 9th the chemical brightness of the south polar regions was considerably greater than that of the north polar regions, while about the equator the brightness was between that of the poles.

In order to show that the sensitive paper, when exposed to ordinary sunlight, becomes homogeneously tinted, the author appends the readings, taken in the way described, from various portions of a piece of the standard paper used for the sun-pictures exposed for some seconds to direct sunlight.

	Reading.	Deviation from mean.
Portion No. 1,	101·4	+0·93
" 2,	100·7	+0·23
" 3,	98·5	—1·97
" 4,	101·6	+1·13
" 5,	99·9	—0·57
" 6.	100·7	+0·23
Mean,	100·47	

The sun-pictures obtained on the sensitive paper must possess only a slight tint, otherwise the differences in shade cannot be accurately observed; they then exhibit a peculiar coarse mottled appearance, which is not due to imperfections in the paper, or the lenses, nor to the action of the earth's atmosphere.

Perhaps these irregular dark and light patches are owing to clouds in the solar atmosphere, and they may have an intimate connexion with the well-known phenomenon of the red prominences.

Mr. Baxendell and the author propose to carry out, according to this method, a regular series of observations of the variation of the relative amounts of brightness on the sun's disk, and they hope before long to be able to present the Society with some further details.

Note on Vegetable Ivory. By Dr. Phipson, F.C.S., &c.

From the London Chemical News, No. 206.

Vegetable ivory is the fruit of *Phytelephas macrocarpa*, a plant allied to the palm trees, common in South America. At the period of maturity the grain forms a hard mass resembling ivory or bone, and which is manufactured into various kinds of ornaments. According to an analysis by Mulder, its composition may be represented by—

$$C_{24}H_{21}O_{21}={}^{2}C_{12}H_{10}O_{10}+HO;$$

Baumhauer obtained a precisely similar result some years later.

I have found that vegetable ivory takes, in contact with concentrated sulphuric acid, a splendid red color, almost equal to magenta. This color, at first pink, then bright red, becomes much deeper and more purple when the acid has been allowed to act for about twelve hours.

This reaction may sometimes be found useful in order to distinguish small pieces of vegetable ivory from the ivory of the elephant's tusk, or from bone, neither of which take this beautiful red color in contact with sulphuric acid.

The analyses quoted above show that the greater portion of vegetable ivory is pure cellulose, but the reaction produced by sulphuric acid proves that other substances are present, for cellulose does not become red with sulphuric acid. Mr. Connel found in 1845, that vegetable ivory contained 81·34 per cent. of cellulose, and that the other substances were gum 6·73, legumine 3·80, albumine 0·42 (that is 4·22 of albuminous substances), oil 0·73, water 9·37, and ash 0·61,=100. Filings of vegetable ivory dried at 140° to 150° C. give 1 per ct. of ash.

Payen found that these filings when boiled with caustic soda took a yellow color, a fact confirmed by Baumhauer, who asserts that potash does not produce any color.

The reaction of sulphuric acid on vegetable ivory has enabled me more than once to distinguish immediately between filings of this substance, and bone of ivory filings. It is owing to the well-known action of this acid upon albuminous substances in presence of sugar, and which has been utilized by Raspail in his microscopic researches. But whether the sugar is formed by the action of the acid in the cellulose, or pre-exists already formed in the substance is of little import. I incline, however, to the first opinion, as the color takes a little time to show itself (five or ten minutes), and as Mr. Connel did not find any sugar ready formed.

I have since observed that the white portion of the cocoa-nut presents a similar reaction with sulphuric acid; the color produced is first pink, then red, reddish purple, and finally, in about sixteen hours, a fine violet.

The colors thus produced with vegetable ivory and cocoa-nut disappear gradually in contact with water, like the fine reddish-brown color produced with essence of turpentine and sulphuric acid.

Gun Cotton as an Explosive Agent.

From the London Practical Mechanic's Journal, October, 1863.

(Continued from page 46.)

Gunpowder and Gun Cotton compared as to Temperature of Ignition.—Nevertheless, it is of interest to introduce something regarding the qualities of gunpowder. Mixed with sulphur, charcoal ignites at a lower temperature than by itself. A mixture of sulphur with charcoal, that had been prepared at temperatures between 150° and 400° C., begins to ignite at 250°; whereas a mixture of sulphur with charcoal, resulting from a temperature between 1000° and 1500°, when heated to 250° only burns off the sulphur, without the combustion of the charcoal.

Charcoal decomposes saltpetre at a temperature altogether different from the degree of heat at which charcoal is produced. Charcoal prepared at temperatures between 150° and 430° decomposes saltpetre at 400°. Charcoal prepared between 1000° and 1800° decomposes saltpetre at a dark red glow heat.

Sulphur decomposes saltpetre at higher temperatures than the charcoal, namely, at about or over 432°. The temperature at which sulphur burns in the air is, according to Violette, 250°, and at this temperature gunpowder also explodes. The explosive quality of various sorts of gunpowder is different, according to the sort of charcoal used, the proportions of the mixtures, and the size and compactness of the grains. Violette observes, that an exact knowledge of the alterations in the composition of the charcoal of powder, in proportion to the temperature of the charcoaling process, must be followed by changes in the respective proportions in the mixture of the gunpowder. It is useless to advert here to the conditions under which charcoal is here produced by the common charcoal burners.

Is Gun Cotton Spontaneously Combustible?—Finally, there still remains the consideration, whether the danger of the spontaneous combustion of gun cotton is so great that it cannot be recommended for shooting purposes.

Spontaneous combustion is well defined by Lieut. Col. Baron von Ebner, his definitions having reference to the military conditions.

Whence, then, have arisen the allegations of spontaneous combustion, and on what basis do they rest? Assuredly, they receive no countenance from the experience acquired by chemists, who in preparing gun cotton have each operated in a different manner. Pelouze maintains that the process of azotizing is finished in a few minutes. The explosive nature of the compound of this chemist, as well as that of Boucher, is therefore attributable to a composition wholly different from that of the Hirtenberg cotton.

Nor are apprehensions of spontaneous combustion deducible from any fair application of theory. Whatever theoretical objections there may be at the present time applicable, were also applicable twelve years ago. In the summer of 1862, the Simmeringer Haide explosion occurred, and we may fairly assume that the list of theoretical assumptions favoring the idea of spontaneous explosion was then pretty well exhausted. In the Simmeringer Haide magazine, however, gun cotton and gunpowder were stored together. To have imputed the explosion to spontaneous ignition of gun cotton, was altogether unwarranted by evidence. With equal show of probability might it have been imputed to the spontaneous ignition of gunpowder.

The latter view might be supported by the experience of numerous powder explosions where not an atom of gun cotton was present, and yet the cause was never ascertained.

Many probable reasons, however, may be adduced favoring the belief that the Hirtenberg cotton did not explode spontaneously; these probabilities rest on grounds both theoretical and practical.

Experimental Proofs demonstrate that Lenk's Gun Cotton is not spontaneously Combustible.—The history of gun cotton as chronicled by chemists and artillerists, short though the history be, is so full of records of explosion under unexpected circumstances, that an unbiassed mind can hardly fail to be impressed with the belief that amongst the ordinary conditions of military practice, there may be some competent to induce the spontaneous combustion of this material. Nevertheless, the experience of Baron Lenk, acquired during a period extending over more than ten years, is more pregnant with reliable testimony than can be found in the entire remaining history of this material.

The *manufacture* of *gun cotton* in *Hirtenberg* consists in a *number* of *perfectly harmless operations;* and it is remarkable that contrary to what happens with gunpowder, if fire be not actually applied, explosion is impossible. All operations are so arranged, that the material acted upon is in a moist or wet condition—hence not explosive. Drying takes place in a capacious building, on every side open to the air. The last process of drying is carried out in the drying chamber, where it is effected by a stove situated on the outside, distributing its heat to the building by earthenware pipes; drying being thus insured through a gentle warmth. The gun cotton next goes either into a magazine to be packed away in chests, or is at once prepared for ammunition.

In *this magazine, Hirtenberg* cotton has been stored for a period of twelve years, and not a single instance of explosion has taken place. How many powder mills have exploded in that time? In Prussia, however, a drying chamber has lately blown up.

Your excellency has officially been informed, that in Prussia they have worked for eight years with gun cotton, and not a single explosion has occurred except the last named. In the Prussian drying chamber referred to, a stove with an iron smoke pipe was used—a sufficient explanation of the misfortune.

During twelve years we have prepared gun cotton at Hirtenberg, for ammunition, that is for yarns, spun ropes, and threads twisted and woven.

One single case of explosion has occurred in the course of Baron Lenk's manufacture, the result of improper speed of working the spinning machinery. Now, the circumstance hardly need be insisted on, that gunpowder as well as gun cotton can be exploded by friction.

Gun cotton has been used for military purposes now more than twelve years; it has also been employed for mining and blasting. It has been subjected to every variety of transport. Packed in black wooden chests, it has been exposed to sunshine for months together; all this without one single accident. In the face of such testimony it cannot be said that gun cotton manifests any tendency to explode spontaneously.

Theoretical Proofs against the Spontaneous Combustion of Lenk's Gun Cotton.—Those who maintain the spontaneous combustibility of gun cotton, must surely be able to base their fears on scientific grounds. Let us now examine the most important of these. And, firstly, as regards cotton itself. Cotton is chemically effected by storage, if packed damp and in bulk; it then gets heated; and examples are not wanting of warehouses burned down through fire generated by the heating of stored cotton. This chemical change in ordinary cotton begins with hydrogen, this element affording to the atmospheric oxygen the first point of attack: hereby occurs heating, with partial oxidation. The oxidation goes on until even the charcoal is affected, and the results may be actual combustion. This occurs not only to cotton, but linen rags, as well as to sawdust, and many other organic substances. The conditions affecting Lenk's gun cotton are almost diametrically opposed to the preceding. Firstly, the three easily removable atoms of hydrogen present in ordinary cotton are disposed of by what may be called azotization; then the long process of acid-steeping involved, oxidizes everything in the cotton that can be azotized; whence it follows that there remains nothing for atmospheric oxygen to act upon. Finally, slow decay, or "*eremacausis,*" to which ordinary cotton is subject, cannot affect Lenk's gun cotton.

Granting that atmospheric oxygen has no influence, still the theoretical allegation may be adduced, that oxygen of water is not without influence. But the Hirtenberg gun cotton lies for several weeks in water; moreover, is finally boiled with weak potash solution. Now, if it be easily changeable by water, it must surely have changed under this treatment: nevertheless, every analysis demonstrates the result to be *tri-nitro cellulose.* It may still be argued, that if oxygen and atmospheric moisture do not produce material changes, still summer heat would do so; but examples such as the black cases which have lain for months in the open air on railway stations, exposed to the direct action of the sun, and the specimen already referred to, where more than one pound of gun cotton was exposed for more than four weeks to the direct action of the sun towards the south in all weathers, retaining still its full explosive properties, demonstrate a sufficient explosive constancy. If, however, gun cotton be prepared otherwise than as in Hirtenberg—or if not prepared in Hirtenberg, according to the special instructions of General von Lenk (of which, until now, no example was before us)—then, no doubt, there might be risk of combustion, as in the case of the French cotton.

How the explosions of the French cottons are to be accounted for.—The explosion can be accounted for in the following way: indeed, it has been mostly explained already.

If the acids are not strong enough, or not permitted to act sufficiently long; if, moreover, the subsequent removal and neutralization of acid be not complete, so that free acids remain; then the atoms of unremoved hydrogen may become the focus of chemical energy, which rising to a sufficient degree of intensity, may result in explosion.

To effect complete permeation of the cotton, is by no means a quick or easy process; and a similar remark is applicable to the removal of lingering acid traces. With reference to these particulars, the statement can, without arrogance, be made that the French gun cotton, compared with that of Hirtenberg manufacture, was not so complete a material, inasmuch as the acid was allowed too short a time for its action upon the cotton wool; probably, too, the subsequent *deacidation* of the French cotton would not have been so fully effected. Thus considered, the assumed great danger of so-called "spontaneous combustion" in reference to Lenk's gun cotton disappears.

It now remains, lastly, for us to inquire into the so-called disruptive explosibility (*vis viva*, or *force brisante*) of gun cotton.

The "force brisante" of Gun Cotton.—This subject, purely artilleristic in its nature, can only be spoken of by chemists, so far as the *force brisante* of gun cotton depends upon a certain chemical condition.

By the expression *force brisante*, we understand the time within which a certain weight of an explosive material in a gun has been converted into a certain volume of gas, or the volume of gas produced under otherwise equal conditions.

An explosive material is therefore the more *brisante* in proportion as the time is short for equal weight and volume of gas developed, or time and weight equal in proportion to the volume of gas is greater.

We, being accustomed to compare every sort of explosive material with gunpowder, say that gun cotton is more *brisante* than gunpowder, because the former develops an equal volume of gas in a smaller space of time; or what is the same, time being equal, a greater bulk of gas, and, therefore, a higher mechanical effect. In the moment of explosion the result exerts a greater pressure on the barrel; hence there is more danger of bursting.

The danger of bursting by equal charges differs relatively according to the thickness of the barrel and the strength of its material to equal resistance of the shot.

Given an explosive substance more *brisante* than gunpowder, we can still fire the two with equal safety by proportionately increasing the strength of the gun. From a consideration of explosive bodies generally, it follows that each particular one, if applied to ballistic purposes demands a peculiar gun. It cannot be reasonably expected that a gun best adapted to the use of gunpowder is also best adapted to the use of gun cotton.

If that construction be discovered, then the higher *brisante* effect of gun cotton may appear as an advantage, instead of being thought a disadvantage.

However, gun cotton can act disadvantageously in another manner on the barrel of a gun, through its higher *brisante* nature.

If we imagine the moment of the discharge of the gun taking place, then the products of the explosion—the gases, are as it were, in a mine, in which one of the resisting sides is movable, viz: the shot. The gases act with equal mechanical power on every side of the barrel in the moment before the shot is about to move. The sides of the gun being at rest (supposing it resists the explosion), it follows that part of the mechanical power is transformed in an equivalent quantity of heat, or, what is the same, the barrel of the gun will become heated; the temperature may be raised thereby so high, that the tin of a bronze barrel may partly melt, or, as we say, the barrel of the gun is "burnt out" after a number of rounds.

That the last-named effect of heat on the barrel is not to be attributed to the chemical composition of gun cotton, but to the transformation of mechanical power into heat, is proved by Lieutenant von Karoly's analysis of the gases of combustion of Lenk's gun cotton, which he made in the Chemical Laboratory of the Engineers' Corps Committee, as may be seen in the report of the Imperial Academy of Science, volume xlvii, page 59, Mathematical and Physical part; and which may be seen in the following table, in which the gases of combustion of powder according to Bunsen (*vide* Poggendorf, 4th series, 12th volume, page 131,) are cited in comparison with those of gun cotton.

Gases of Combustion. Volume per cent.		Bunsen.			Karoly's.
		Sporting.	Rifle.	Common Powder.	Gun Cotton.
Nitrogen, .	N.	41·1	35·3	37·6	. 12·7
Carbonic Acid, .	$C.O._2$	52·7	48·9	42·7	. 20·8
Carbonic Oxide,	C.O.	3·9	5·2	10·2	. 29 0
Hydrogen, .	H.	1·2	6·9	5·9	. 3·2
Sulphur and Hydrogen,	H.S.	0·6	0·67	0·86	Carbon, 1·8
Oxygen, .	O.	0·55	. .	. .	Water, 25·37
Light Carburetted Hydrogen Gas, .	.	0·00	3·02	2·7	. 7·2

If we compare the gases of gunpowder with those of gun cotton, we easily see that the chemical action of the product of combustion of gun cotton on the sides of the barrel—if there exists any action at all—must be smaller than with the use of gunpowder, because they are less oxidizing gases than those of gunpowder.

Should, therefore, bronze barrels be "burnt out" by the use of gun cotton, cast steel may be then used instead of bronze, which, in fact, has been successfully done.

Moreover, bronze gun barrels have withstood a sufficient number of rounds by using an adequate charge of gun cotton with elongated cartridges. In this way no alteration of the bore prejudicial to the correctness of aim has taken place.

From the steel barrel of a rifle, forty rounds have been fired with gun cotton cartridges, which have hit the target 300 yards distant in an unexceptionable manner. After the said number of rounds, the barrel was internally as clean and polished as a mirror. It appears, then, that this problem is solved in a general and satisfactory manner.

Application of Gun Cotton to civil mining and in warfare.—Gun cotton is also used for mining purposes and mining warfare. On this subject nothing but what is favorable has been reported by the Imperial Engineers (*vide* communications of the R. K. Engineers' committee, 1861, 1st volume, by Moritz Baron von Ebner, Colonel of the Engineers).

However, it is said that the gases of gun cotton were more poisonous in mines than those of gunpowder, and, therefore, the use of gun cotton for mining warfare is not to be recommended.

If we compare the results of Lieutenant Karoly's analysis of the combustion gases of gun cotton with those of gunpowder as above given, we observe that both of them contain irrespirable gases; further that they contain qualitatively the same sort of irrespirable gases; and although the relative quantities of some of the gases from powder and gun cotton are different, the effect of those gases leads to the same practical result, viz: that after blowing up a mine, one cannot without danger approach the spot of the explosion before renewing the air by ventilation.

In this respect we may say that the gases of gun cotton will be more quickly removed by ventilation than those of gunpowder, because the first named contain a greater quantity of gases easily dissipated; since 100 pounds of gunpowder contains 68 pounds of fixed solid matter, which alone suffices to make respiration almost impossible.

It is not probable that an explosive compound will be found which will produce any other but irrespirable gases.

It is one and the same in practice, whether a cellar contains 40 per cent. of carbonic acid and 10 per cent. carbonic oxide, or 30 per cent. carbonic oxide and 20 per cent. carbonic acid, inasmuch as no one could without danger of suffocation, enter such a cellar.

Both the gases of gun cotton and of gunpowder, according to Karoly's, may be ignited by a match.

Recapitulation and conclusions.—We believe we have in the preceding furnished replies to your excellency to all the important questions which chemists may be called upon to decide prior to determining whether or not General Lenk's gun cotton can be used as an explosive material for warfare; and we will conclude our report with the following general remarks:—

General Baron von Lenk's gun cotton is almost wholly made up of tri-nitro cellulose; it is manufactured in the imperial factory at Hirtenberg in such manner that one invariable product results.

Lenk's gun cotton remains unaltered under circumstances that would render gunpowder totally useless. It is not subject to any notable alteration, nor is it prone to spontaneous combustion. Its temperature of ignition is somewhere between 130° and 160° C., sufficiently high to remove apprehension. Its *vis viva*, or *force brisante*, can be moderated without difficulty if required, or else special cannon and rifle barrels may be adapted to it.

The dangers in regard to mining warfare are comparatively and qualitatively the same as with the use of gunpowder,—the gases of both being irrespirable.

The manufacture of gun cotton at Hirtenberg is provisional, and General Baron W. von Lenk will know best how to devise improved mechanical apparatus when the erection of a proper factory shall be resolved upon.

According to our experience up to the present time, we recognise in the *gun cotton from Hirtenberg an improved explosive compound having many and great advantages, several of which gunpowder from its very nature can never possess.*

A Novel Application of Water Power.

From the London Mechanics' Magazine, October, 1863.

Just forty years since, M. Fourneyron commenced a series of experiments in water power, which resulted in his invention of the turbine or horizontal water-wheel. Since that period considerable improvements have been made in the turbine by different persons, the chief and most useful having been effected by Mr. Schiele, of Manchester, whose ingenious applications of mechanical curves seem to have been fully adapted by him for the production of this form of motive power. One form of his arrangement for supplying power we have recently seen (working the bellows of a powerful organ) at the residence of a citizen of Manchester, where the impression was given that, if all the results achieved by Mr. Schiele be equally successful, a new feature will be rapidly developed in applying water power, especially in cases where a small amount of power may be required at irregular periods; as in the case of working the bellows of organs, driving small lathes, fans for ventilation, printing and other presses, sewing machines, washing machines, &c. In the house referred to, a water-wheel, 4 feet in diameter, consuming 15 gallons of high-pressure water per minute, formerly employed to work the bellows of an organ in the drawing-room over the cellar wherein the water power was produced, has been replaced by a turbine only 1¼ inches in diameter, with a 3-inch case 1¼ inches wide, supplied by a ⅜-inch pipe, and consuming less than a gallon of water per minute. An ingenious and yet very simple economical regulator, invented by Mr. Eccleston, organ builder, of this city, works in connexion with the apparatus just mentioned, by means of which the organist may easily supply his instrument with the required wind by simply turning a handle near the organ. By availing themselves of the ample supply of high-pressure water secured to the city by our Corporation, all persons using machines requiring a small amount of power appear now to have supplied to them by this invention the means of working their machine with no trouble and at a trifling cost; while at the same time this kind of turbine appears to be equally well adapted for turning large mills and works, even when they require several hundreds of horses' power. Orders are now being executed by Messrs. Schiele and Co. for the construction of 50 small turbines, to be used as direct-action fans (the turbine and fan being on one spindle) for the production of the new gas obtained from petroleum. Several powerful turbines will shortly be at work in this locality, when our readers will be able to see and judge for themselves of the extraordinary yet simple effect of this new water engine, which seems to be equally suited for the requirements of the sewing machine in a lady's boudoir, the washing machine and mangle in the laundry, or the hydraulic press and hoist in our huge ware-houses. In fact, wherever our Corporation water-works will enable persons to turn a water tap, and thus to supply at a moment's notice the power required, these machines will be available; while all the risk from fire and the cost and trouble of steam boilers and engines will be avoided.—*Manchester Guardian.*

Extraction of Iron.

From the London Chemical News, No. 206.

To the Editor of the Chemical News.

SIR:—I find in your paper of October 31, an article by M. A. L. Fleury on the "Extraction of Iron from Cinders of Puddling and Reheating Furnaces," and as the statements therein contained might mislead iron masters into the belief that his invention is a new one, I deem it my duty to address to you the following remarks:—

In the year 1854 I devoted much time to ascertaining the best methods of employing cinders from puddling and refining furnaces, so as to enable iron masters to use them in larger proportion on their blast furnaces without injury to the quality of the iron produced; and in August of the same year I took out a patent to carry out my views, which is identical with that of M. Fleury. In my specification I stated "The slag or cinder, either before or after being calcined or roasted, is ground into coarse powder and mixed with half its weight of slacked lime, and made into a thick paste, which is formed into lumps or bricks with or without coal-dust or charcoal, and is introduced, dried, or calcined or not, into the blast furnace, and worked in the usual way."

I soon found that this process was not practicable, owing to the enormous expense of grinding the cinders, and afterwards mixing them with slacked lime and making the whole into bricks, and lastly, because the constant vibration which exists in blast furnaces, owing to the working of the blast engine, caused the bricks to fall into powder and to interfere with the draft, and consequently to impede the working of the furnaces.

Therefore, in September, 1855, I took out a second patent for fusing the cinders with about 20 per cent. of quicklime, 30 per cent. of slacked lime, or 50 per cent. of carbonate of lime, previously to using the cinders on the blast furnace. This process I carried out with perfect success on the continent and in South Wales; by this process the silica, sulphur, or phosphorus contained in the slag are retained by the lime, and pass away in the cinder of the blast furnace, whilst the oxide of iron thus liberated is reduced with great facility to a metallic state. I am aware that cinder or slag and lime had been used on the blast furnace for a long period, but my improvement consists in combining cinders and lime in proper proportions previously to placing them on the blast furnace, thus securing a perfect combination of the cinder with its flux, so that when it comes into contact with the reducing agents in the blast furnace, the oxide of iron is at once liberated from the impurities with which it is united.

As to the value of chloride of sodium for purifying iron, I would also refer M. Fleury to a patent which I secured on October 30, 1851, for the use of chloride of sodium as a means of removing sulphur either from iron ores or from cokes. The value of this process was fully tested on a practical scale at several iron works, and also in a series of experiments conducted at the establishments of the following eminent engineering firms, with results which I append :

Experiments conducted at various works showing the comparative bearing strength of iron bars one inch square, four feet six inches between the bearings, melted by the ordinary and by my purified coke. These experiments were upon various qualities and descriptions of iron, and varied in every possible way, in order that the results might be relied on.

At the Works of	Mean breaking weight of bars melted by		Difference in favor of purified Coke.
	Ordinary Coke.	Purified Coke.	
	lbs.	lbs.	per cent.
Fairbairn and Sons, Manchester, .	427	512	20
J. & W. Galloway, " .	547	620	13
Fox, Henderson & Co., Birmingham, No. 3 iron,	435	514	18
" " " " No. 2 "	317	459	10
Hibbert, Platt & Co., Oldham, .	399	552	10
Monkland & Co., . . .	578	641	10
Ivicey & Co., Skinnerburn, .	616	*741	20
Newcastle, ...	658	*716	9
Elswick Foundry, . . .	695	*863	24

Messrs. Fox, Henderson, and Co. paid particular attention to the quantity of the gas given off during the process of casting the bars, and report as follows :—

" We could easily smell the sulphur given out from the top of the cupola, and also from the tapping hole, in which ordinary coke was used, but there was no smell whatever, save a little of salt, from the one which had patent coke in it."

* Were bars of three feet ten inches and four feet only in length.

Experiments made at the Victoria Works, Ebbw Vale, for the purpose of testing the yield of metal smelted with the ordinary process, and by Mr. Calvert's improvement.

By ordinary process.

	Charges.	Yield tons.
No. 3 furnace, week ending June 19, . .	481	160
" " " 26, . .	468	174
" " July 3, . .	472	182
By Mr. Calvert's process.		
Week ending July 10, ...	458	183
" " 17, ...	492	197
" " 24, ...	488	201
" " 31, ...	496	202

(Signed) Thos. Harris.

I am, &c. F. Crace Calvert.

Royal Institution, Manchester.

On Aluminium. By Mr. I. L. Bell.

From the London Athenæum, Sept. 1863.

The author said—" The progress of the manufacture of this, so far as the arts are concerned, new metal has scarcely been such as to require much to be added to the researches bestowed upon the process by the distinguished chemist, M. St. Cairi Deville, of Paris. Upon the introduction of its manufacture at Washington, three years and a half ago, the source of the alumina was the ordinary ammonia alum of commerce, a nearly pure sulphate of alumina and ammonia. Exposure to heat drove off the water, sulphuric acid, and ammonia, leaving the alumina. This was converted into a double chloride of aluminium and sodium by the process described by the French chemist and practised in France, and the double chloride subsequently decomposed by fusion with sodium. Faint, however, as the traces might be of impurity in the alum itself, they, to a great extent, if not entirely, being of a fixed character when exposed to heat, were to be found in the alumina, from which, by the action of the chlorine on the heated mass, a large proportion, if not all, found their way into the sublimed double chloride, and once there, it is unnecessary to say that under the influence of the sodium, any silica, iron, or phosphorus found their way into the aluminium sought to be obtained. Now, it happens that the presence of these impurities in a degree so small as almost to be infinitesimal, interferes so largely with the color as well as with the malleability of the aluminium, that the use of any substance containing them is of a fatal character. Nor is this all, for the nature of that compound which hitherto has constituted the most important application to this metal —I mean aluminium-bronze—is so completely changed by using aluminium containing the impurities referred to, that it ceases to possess any of those properties which render it valuable. As an example of the amount of interference exercised by very minute quantities of foreign matters, it is, perhaps, worthy of notice that very few varieties of copper have been found susceptible of being employed for the manufacture of aluminium-bronze; and hitherto we have not at Washington, nor have they in France, been able to establish in what the difference consists between copper fit for the production of aluminium-bronze, and that which is utterly unsuitable for the purpose. These considerations have led us, both here and in France, to adopt the use of another raw material for the production of aluminium, which either does not contain the impurities referred to as so prejudicial, or contains them in such a form as to admit of their easy separation. This material is Bauxite, so called from the name of the locality where it is found in France. The Bauxite is ground and mixed with the ordinary alkali of commerce, heated in a furnace. The metal is so extensively used in the arts as to keep the only work in England, namely, that at Washington, pretty actively employed. As a substance for works of art, when whitened by means of hydrofluoric and phosphoric acid, it appears well adapted, as it runs into the most complicated patterns, and has the advantage of preserving its color, from the absence of all tendency to unite with sulphur or become affected by sulphuretted hydrogen. A large amount of the increased activity in the manufacture referred to is due to the exceeding beauty of its compound with copper, which is so like gold as scarcely to be distinguishable from that metal, with the additional valuable property of being nearly as hard as iron."

Proceedings British Association.

On the Expansive Energy of Heated Water. By W. J. MACQUORN RANKINE.

(Read to the Institution of Engineers, Scotland.)

From the Lond. Civ. Eng. and Arch. Journal, Nov., 1863.

As the question of the quantity of mechanical energy which a given weight of water, heated to a given temperature, is capable of exerting in the act of partially evaporating without receiving heat, until it falls to a given lower temperature, has been raised in connexion with recent researches as to the bursting of steam boilers, I may point out that the complete solution of that question for any given liquid, together with a numerical example in the case of water, is given under the head of Proposition XVII, of a paper on Thermodynamics, which was communicated to the Royal Society in December, 1853, read in January, 1854, and published in the "Philosophical Transactions" for 1854.

That solution is expressed by the following formula (page 161, equation 65):—

Energy exerted by each pound of fluid

$$=\mathrm{K}\left\{ t_1-t_2\left(1+\text{hyp.}\log.\frac{t_1}{t_2}\right)\right\} \quad . \quad . \quad (1)$$

in which K denotes the dynamical values of the specific heat of the liquid, being the product of its specific heat expressed in the ordinary way by "Joule's Equivalent;" t_1 and t_2 the initial and final absolute temperatures; the absolute zero being 274° Centig., or 493°2 Fahr. below the melting point of ice.*

Another equation (equation 63 of the paper) gives the following value for the excess of the final volume to which the mixed liquid and vapor expand, above the original volume of the liquid:

$$\frac{\text{K hyp. log.} \frac{t_1}{t_2}}{\frac{dp_2}{dt_2}} \quad . \quad . \quad . \quad . \qquad (2)$$

in which $\frac{dp_2}{dt_2}$ denotes the rate at which the pressure, in pounds on the square foot, varies with temperature at the final temperature.

When applied to the water in a steam boiler, these equations take the following form:—

The value of K for liquid water is 772 foot-pounds per degree of Fahrenheit in a pound of water, or
1389·6 foot-pounds per Centigrade degree in a pound of water, or
423·55 kilogrammetres per Centigrade degree in a kilogramme of water.

The final absolute temperature is 212° F. + 461°·2 = 673°·2 F.

The corresponding value of $\frac{dp}{dt}$ for Fahrenheit's scale and British measures, is 42: and 772 ÷ 42 = 18·38.

Let T denote the initial temperature on Fahrenheit's ordinary scale; so that $t_1 = \text{T} + 461°{\cdot}2$. Then

Energy in foot-pounds exerted by each pound of water

$$= 772 \left\{ \text{T} - 212° - 673°{\cdot}2 \text{ hyp. log.} \left(\frac{\text{T} + 461°{\cdot}2}{613°{\cdot}2} \right) \right\}^{\dagger} \quad . \qquad (3)$$

Final Volume of expansion of mixed water and steam, in cubic feet per pound,

$$= 18{\cdot}38 \text{ hyp. log.} \left(\frac{\text{T} + 461{\cdot}2}{673°{\cdot}2} \right) \quad . \quad . \quad . \qquad (4)$$

It is worthy of remark, *that the energy developed depends solely on the specific heat of the substance in the liquid state, and the initial and final temperatures*, and not on any other physical property of the substance.

The following table gives some results of the formulæ. The first column contains the temperature on the ordinary scale of Fahrenheit, with intervals of 36° Fahr. = 20° Centig. The second column contains the expansive energy of one pound of water, in foot-pounds. The third column contains the velocity, in feet per second, which that energy would impress on a projectile of the weight of the water itself: that is, one pound. The fourth column, the final volume of expansion of the water and steam, in cubic feet per pound. For convenience, a fifth column is added, containing the initial absolute or total pressures in pounds on the square inch. The last line of the table has reference to the case in which the water would be totally evaporated.

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* In the original paper the absolute zero of heat was assumed to be 272½ Centig. below the melting point of ice. The value now adopted, 274° Centig., is deduced from later experimental data.

† This agrees with the formulæ given by J. B. in the "Engineer," of the 2d. October, 1863, page 200.

TABLE I.

Initial temperature.	Energy.	Velocity.	Final expansion.	Initial absolute pressure.
Fahr.	ft. lbs.	ft. per sec.	cubic ft.	lb. per sq. inch.
212°	0	0	0	14·70
248	726	214	0·95	28·83
284	2779	423	1·87	52·52
320	6052	624	2·73	89·86
356	10422	819	3·56	145·8
392	15826	1010	4·36	285·9
428	22156	1194	5·11	336·3
. .	. .	. .	. .	. .
about 2360	about 912500	about 7666	26·36	unknown.

In the absence of logarithmic tables, the following approximate formulæ may be used for temperatures not exceeding 428°.

$$\text{Energy, nearly} = \frac{772(\mathrm{T}-212^\circ)^2}{\mathrm{T}+1134^\circ{\cdot}4} \quad . \quad . \quad . \quad (5)$$

$$\text{Expansion, nearly} = \frac{36{\cdot}76(\mathrm{T}-212^\circ)}{\mathrm{T}+1134^\circ{\cdot}4} \quad . \quad . \quad (6)$$

Glasgow, October 5, 1863.

NOTE—*Added* 19*th October*, 1863.

In explanation of the formulæ and table it may be added, that the mechanical energy in column 2 is the equivalent of the heat which disappears during the process, being the difference between the whole heat expended and the latent heat of that portion of the water which at the end of the process is in the condition of steam at atmospheric pressure.

TABLE II.

Initial temperature.	Portion of the water which remains liquid.	Energy possessed by that liquid water.
Fahrenheit.	lb.	ft. lbs.
212°	1·000	0
248	0·964	700
284	0·931	2587
320	0·897	5429
356	0·865	9015
392	0·835	13215
428	0·806	17828
. .	. .	. .
about 2360	0	0

For the information of those who consider that the liquid portion of the water, owing to small compressibility, acts like a volley of hard projectiles, a table (II.) is added, showing, for each of the initial temperatures in the previous table, what fraction of a pound of water continues in the liquid state, and how much of the energy developed is possessed by that liquid water.

In the formulæ and tables it has not been considered necessary to take into account the small increase which the specific heat of water undergoes as the temperature rises.

On the Behavior of Chloride, Bromide, and Iodide of Silver in Light, and the Theory of Photography. By HERMAN VOGEL.

From the London Chemical News, No. 205.

This paper contains the results of researches extending over three years, and undertaken by the author to determine the following questions:—

1. What effect has light on chloride, bromide, and iodide of silver?
2. What influence on the action have foreign substances, such as water, acids, free silver salt, and organic matters, &c.?
3. What changes do the silver compounds produced by light undergo in the developing process?

To answer the first question, what effect has light on chloride, bromide, and iodide of silver, many experiments have been made by other chemists, principally with the chloride of silver, some with the iodide, and very few with the bromide. The author briefly refers to these, which are of some importance in the history of photography.

Scheele was, perhaps, the first (1777) who studied the action of light on the chloride of silver. He noticed that it blackened in sunlight, and left metallic silver after treatment with NH_3; hence he concluded that light had decomposed the chloride into Ag and Cl.

A. Vogel and Wetzlar, however, found that the blackened AgCl could not be decolorized by heating with NO_5, nor was any silver dissolved, they therefore inferred that sunlight decomposed the chloride, into Cl and a subchloride. Wetzlar further remarked that the chloride blackened under nitric acid. Wittstein also observed that nitric acid dissolved no silver from blackened chloride.

Robert Hunt, on the contrary, maintains that metallic silver is formed on the surface of photographic paper together with Ag_2Cl and oxide of silver.

Guthrie and Draper affirm that AgCl is decomposed into Ag and Cl, and Guthrie supposes that the Ag is reduced to a passive state in which it is insoluble to NO_5. Draper says that the properties of the free silver are so changed that one might believe in the transmutation of the metal.

Spiller asserts that by a year's exposure to light the AgCl is changed into a body, the specific gravity of which favors the opinion that AgCl is decomposed into its constituents. Malone affirms the same, and supposes that a red and brown silver may exist.

Dawson, from the results of a long series of experiments, concluded that the final action of light on AgCl was the formation of metallic silver, and that the reduction was gradual, the chloride passing through the stages AgCl, Ag_2Cl, Ag_3Cl, Ag_4Cl, &c.

Davanne and Girard assert the complete decomposition of the chloride.

Hardwich, on the contrary, contends for the formation of a subchloride.

The above opinions, and many others that might be quoted, show the different views on the nature of the change produced by light on chloride of silver.

With regard to bromide, Berthier says that this compound changes to a grey color, and blackens more slowly than the chloride.

Monckhoven asserts that it changes more rapidly than the chloride.

More extended researches have been made with the iodide by Daguerre and others. Moser and Draper agree that iodized silver plates undergo no chemical change on exposure. Schnauss supposes that iodide of silver is not chemically changed, but acquires the property of attracting particles of reduced silver "by a sort of electrical tension." He showed that no picture could be produced by iodide of silver alone, but that it was developed by a silver precipitate deposited on the exposed portions of the iodide. Later he conclusively proved that there are two modifications of the iodide—one sensitive the other non-sensitive. The first is produced when the iodide is precipitated in an excess of silver solution; the second when an excess of iodide of potassium is used. He states that the sensitive iodide becomes brown in the light, but seems to believe that this photographic property is owing to the presence of a trace of $AgONO_5$, which cannot be removed even by long washing. He thinks that the combination $AgI+AgONO_5$, which may be crystallized out of solutions containing AgI, is the really sensitive agent. He supposes, too, that a decomposition of the iodide into free iodine and a subiodide takes place on the long exposure of light.

Sutton asserts the non-sensitiveness of AgI prepared with an excess of KI, and is opposed to its chemical decomposition. Pure iodide he supposes to be insensible, and attributes the sensitiveness to the presence of a trace of nitrate. But he believes that the chloride and bromide are decomposed, the silver reduced, and Cl and Br set free. Davanne maintains that the iodide is decomposed into iodine and silver. Hardwich and Monckhoven have, on valid grounds, combated this view.

After this short review of the investigations of other chemists, the author proceeds to detail the results of his own *experiments with pure chloride, bromide, and iodide of silver.*

He prepared AgCl by gaslight, by precipitating $AgONO_5$ with HCl, and washing the precipitate until the washings gave no trace of Cl. AgBr and AgI were made by precipitating $AgONO_5$ with an excess of KBr and KI. The chloride obtained as above was white, the bromide was pale yellow, and the iodide straw-colored. After careful washing they were dried and kept in the dark.

To test their behavior towards light, small quantities were placed in test-tubes by lamplight, the salt was rubbed to a powder with a glass rod, and the tube was sealed or closed with a cork; the tubes were then exposed to light. A change of color was soon observed in the chloride and bromide; the former became violet, the latter grey. The color deepened with time, and reached a maximum in several days; the AgCl was then of a brownish violet color, and the bromide an impure pale greyish violet. The coloration was only superficial, the interior remaining white.

Iodide of silver prepared as above underwent no change of color in the course of some months exposure to intense light.

In most text-books it is said that AgCl and AgBr become black in the light. This is not correct; even after exposure for years they only become colored as above. The assertion of Seebeck that AgCl only blackens in the presence of moisture is equally incorrect. Perfectly dry and even fused chloride turns violet on exposure.

The change of color is always accompanied by a chemical change, in which chlorine and bromine are set free. Both of these I recognised by their odor, and by the reaction on iodide of potassium starch paper. A tube with AgCl and another with AgBr, having each a strip of paper fixed in the cork, were exposed to sunlight simultaneously. The paper in both instances became colored, in the case of the bromide very quickly, but with the chloride more slowly. The experiment repeated under the diffused light of a blue sky gave the same results. Experiments were also made with fused AgCl and AgBr. These were sealed in tubes; after several days exposure the tubes were opened, and a strip of the iodized paper introduced. The paper was only faintly colored by the chloride, but much more distinctly by the bromide. From these experiments we must conclude that by exposure to light AgCl and AgBr are decomposed, and chlorine and bromine set free, the decomposition taking place more quickly with the AgBr than with the AgCl.

The question now to be solved is what compound remains behind. As only pure chloride and bromide were exposed, and free bromine and chlorine were recognised, either (1) the compounds must have been decomposed into their constituents; or (2) a sub-chloride and sub-bromide must have been formed with the evolution of free chlorine and bromine. In the first case free silver must have been present in the compounds exposed. To detect this, chloride and bromide which had been exposed for months, were boiled in nitric acid (sp. gr. 1·2), and the clear liquor tested with dilute HCl; not a trace of silver was discovered in either case. From this it may be concluded that a sub-chloride and a sub-bromide are formed by exposure.

The objection of Guthrie that silver is reduced to a passive state is scarcely worth refutation, and the assertion of Hunt that oxide of silver is formed is contradicted by the above experiments. Spiller's assertions are of too hypothetical a nature; he expresses conjectures which are only half supported by facts. The free silver obtained by Dawson, who exposed chloride of silver with free silver solution, or with solution of chloride of sodium, can be easily accounted for; nitrate of silver is decomposed in the light, and Wetzlar has shown that chloride of sodium has the power of decomposing Ag_2Cl with the separation of Ag.

The quantitative composition of the sub-chloride and sub-bromide of silver formed by exposure to light it is impossible to determine, since we are unable to separate the part affected by light from the unchanged AgCl and AgBr. The qualitative reactions, on the other hand, are of importance. Ammonia poured over deeply-colored AgCl dissolves the greater part, leaving only some grey granules. These, on pressure with a glass rod, become silver white, with a metallic lustre; and, as they dissolved when heated with NO_3, are metallic silver. The same reaction is observed in the case of the brown chloride of silver obtained by Wetzlar by the action of perchloride of iron on silver leaf, and also by Wohler by the action of HCl on protocitrate of silver. These bodies give no silver to nitric acid, but are decomposed by ammonia, chloride of silver being dissolved, and metallic silver remaining. The indifference towards nitric acid proves that these bodies cannot be regarded as mixtures of chloride and metallic silver.

That the compound obtained by exposure is really a mixture of Ag_2Cl and AgCl the author considers satifactorily proved by the fact that he obtained a body exhibiting precisely the same color and behavior towards nitric acid and ammonia on treating Rose's compound of ferroso-ferric oxide and protoxide of silver with hydrochloric acid. He prepared a similarly violet-colored chloride by treating blackened peroxide of silver (which he shows to be a mixture of AgO_2 and AgO) with hydrochloric acid, and this compound behaved toward nitric acid and ammonia in precisely the same way. Carbonate of silver, after exposure to light, also furnished a chloride containing Ag_2Cl, and the author thinks it extremely probable that all silver salts reducible by light are first reduced to a state of protoxide, or the analogous haloid salt, and that any metallic silver is the result of a secondary decomposition.

A sub-bromide, similar to the sub-chloride of silver, may be prepared by treating Rose's compound with dilute hydrobromic acid. A pale grey violet body is thus produced exactly like the body produced by light. On boiling with NO_5 it became grey, as happened with exposed AgBr. When NO_3 was poured over the mass boiled in NO_5 it took a darker grey color, and a grey powder settled at the bottom. This dissolved in NO_5, and gave a silver reaction. The exposed AgBr behaved in the same way.

We see therefore that the behavior of AgBr in light is exactly analogous to that of AgCl—that by exposure a body is produced which shows the same behavior towards reagents as the exposed AgCl—justifying the conclusion that it contains a sub-bromide

(Ag_2 Br). The decomposition with NH_3 appears to be more difficult in the case of sub-bromide than the sub-chloride, owing, most likely, to the more difficult solubility of the AgBr in ammonia.

I come now to the iodide of silver. I have already remarked that this suffers no *visible change* in the light. The question now is whether it undergoes any *chemical* alteration similar to that which takes place with chloride and bromide? I sought next to ascertain whether any iodine was set free. With this object I dropped freshly-prepared thin starch paste on AgI, which had been a long time exposed in a closed glass tube. No reaction followed. With AgI covered with starch paste and exposed to intense sunlight no reaction was observed, nor could free iodine be recognised with sulphide of carbon. I repeated this test with starch paste containing KI, but even with this was unable to discover any trace of free iodine. From this experiment I must come to the conclusion that by the exposure of AgI to light no iodine is set free. If therefore AgI suffers any decomposition it can only consist in the formation of free *silver* or a *subiodide*, and of a *superiodide.*

The information can only be ascertained in this by the solution of the undecomposed AgI in $NaOS_2O_2$; by this means I obtained *no residue* of free silver.

The formation of free silver in this case is also improbable, as it must have been recognised by the surface becoming grey. There remains therefore only the second supposition—the formation of a sub and superiodide. The formation of the latter, however, is very improbable. Notwithstanding their much stronger affinities Cl and Br have but a slight disposition to form superchlorides and superbromides, the disposition is still less in the case of iodine. We know, for example, perchloride of iron and perchloride of copper, but not periodide of iron and periodide of copper in a free state. In experiments on the formation of these bodies only iodide is produced, and iodine set free. I cannot therefore bring myself to believe in the formation of a superiodide of silver, the less so as it is very improbable that in case it existed it could withstand the action of iodide of potassium.

But if no superiodide is formed, and no iodine is set free, the formation of a subiodide or metallic silver is also impossible; the conclusion therefore is that on the exposure of AgI *no chemical decomposition takes place.*

I have hiherto described the behavior of the haloid silver salts prepared with an excess of the *precipitant* (HCl,KBr, KI). *The results obtained with haloid salts, prepared with an excess of silver salt, are somewhat different.* These salts were prepared by lamplight, and washed until the washings gave no silver reaction. I shall denote these salts $AgCl\beta AgBr\beta$ and $AgI\beta$, and those before described $AgCl\alpha AgB\alpha$ and $AgI\alpha$. $AgCl\beta$ and $AgBr\beta$ do not differ in external appearance from $AgCl\alpha$ and $AgBr\alpha$, but $AgI\beta$ is of a deeper yellow color than $AgI\alpha$. More characteristic was the difference of the behavior of the salts in the light. $AgCl\alpha$ *decomposes more slowly* in the light than $AgBr\alpha$; $AgCl\beta$, on the contrary, *decomposes quicker* than $AgBr\beta$. I ascribe the quicker decomposition of the $AgBr\alpha$ compared with the $AgBr\beta$ to the finer division of the former.

With regard to coloration, $AgCl\alpha$ and $AgCl\beta$, as well as $AgBr\alpha$ and $AgBr\beta$, behave exactly alike. It is otherwise with $AgI\beta$. *While* $AgI\alpha$ *does not show the least change of color in light,* $AgI\beta$ *becomes decidedly grey with a greenish tinge.*

On experimenting with $AgI\beta$ as with $AgI\alpha$ to ascertain whether iodine was set free by the exposure of the former, the author was brought to the conclusion that neither $AgI\alpha$ nor $AgI\beta$ underwent any change in the light.

(To be Continued.)

New Method of Engraving and Multiplying Prints, &c.

From the London Art Journal, January, 1864.

Of the many processes that have been proposed for expediting the production of engravings, none are so likely to attain the desired end as a method named after its ingenious inventor, the Proces Vial, many and various results of which are in the possession of Messrs. Hunt and Davies, in Searle Street, Lincoln's Inn Fields. After the admiration excited by a really simple and beautiful invention such as this, comes the question of its practical utility, and this is at once obvious. The prints we have seen from plates prepared by M. Vial's process, are in spirit and character so different, that they could not be pronounced as the results of one and the same method of re-production, some being marked "instantaneous," others, "five" or "ten" minutes, meaning that the design was, so to say, engraved, and the print produced, within a few minutes.

To begin with a fine line engraving. There is a small plate, of which the subject is a sportsman shooting a hare, in every respect like a proof from a plate engraved by a masterly hand: every blade of grass, every weedy tuft, is there, with the same precision that distinguishes the print taken from the plate in the usual way. Perhaps even clearer than this, is a print of a fawn in a thicket, which it is impossible to fancy other than a finished proof from a highly-wrought copper-plate. To artists, the most interesting capability of the invention is its power of transferring, in a few minutes, impressions of a drawing or sketch to a steel plate, line for line, touch for touch. M. Gerome, the celebrated French artist, made, in the presence of the French Commissioners of Fine Art, a sketch of the head of a dromedary, which was prepared and printed from in a few minutes. The sketch was drawn with lithographic chalk. There is by Jules David a sketch something between an etching and a wood-cut: the subject, a French cottage girl hearing a child read; also a dog by another artist, drawn with chalk and touched with India ink, wherein the spirit and manner of the drawing are so perfectly maintained, that it is difficult at first to determine it other than the drawing itself.

It will at once strike the experienced artist that it is by chemical agency the plate is prepared with such rapidity; and the means are so simple, that it is a matter of surprise it has not been before made subservient to the re-production of drawings, sketches, and all kinds of designs to which the invention is applicable, for, from what we have said, it will be understood that the range it embraces is very wide. The preparation of the plate, and the impression taken from it, is, as we have said, effected in a few minutes. The inventor describes his process as "instantaneous;" in certain cases it is, and in all cases very rapid. An artist makes a drawing with lithographic chalk or ink on a steel plate; this is immediately immersed in a bath of a solution of copper, which leaves a thin coat of the metal on all exposed portions of the steel, but "bites in" the lines or touches left by a brush or crayon point. The action of the solution is so rapid, that experience is necessary to determine the point at which the corrosion must be arrested. In lithography a like means is used, but with a reverse result, inasmuch as the printing-surface remains in relief. Not only can the drawing be made directly on the steel and reproduced therefrom, but a drawing made on paper could be transferred to the steel without any alteration. We are told that the most elaborate engraving or lithograph can be reproduced on a steel plate in from ten to thirty minutes from a paper proof, without injury to the original. Thus it is impossible to say what limit can be set to the extensive utility of the invention. And now as to cost—that is said to admit of a reduction of sixty per cent. from the average expense of the production of engravings. It will be understood that in the multiplication of all fine works, as, for instance, engravings, the plates must first be engraved in the usual manner before they can be transferred; but as far as we have seen and understand the process, it must greatly reduce the cost of a large class of works for which the various and ordinary manners of engraving and printing are now employed.

Casting of the 20-inch Gun at Fort Pitt Foundry, Pittsburgh.

The 20-inch gun was cast at the Fort Pitt foundry, Pittsburgh, on Thursday noon. This immense gun, which was first suggested by Major Rodman in his report on the trial of the first 15-inch gun, has long been a matter of theory. Preparations for its manufacture were begun in May, 1863, and were continued from time to time as the exigencies of the service permitted.

The model is an enlargement of the first 15-inch gun. The length of the rough casting is 26 feet; its maximum diameter 66 inches; and its weight 180,000 pounds. The length of the finished gun will be 20 feet 3 inches; its maximum diameter 64 inches; and its weight 115,000 pounds. The diameter of the rough casting at the muzzle will be four feet, and that of the finished gun at the muzzle 34 inches. The whole length of the bore is 210 inches. The bore will terminate in a semi-ellipsoid, whose major axis will be thirty inches, and whose minor axis will be twenty inches.

The mould consists of a flask (made in four pieces, bolted and clamped firmly together), and of the sand which it contains, which forms the matrix of the gun. The flask weighs twenty-eight tons, and the sand ten. The mould is made by placing the flask over a wooden pattern, having the form of the rough casting, and then ramming the sand between the pattern and the flask. The pattern is then withdrawn, the mould is "slicked" or smoothed over, and then washed with a black coating made of ground coal. The mould is then placed in an oven, and baked until it has the hardness of an ordinary soft red brick; this mass of sand was rammed, finished, blacked, and placed in the oven, by ten men, in twenty hours. The flask being removed from its ovens, and clamped firmly together, is placed vertically in a pit, made for its reception. Thus disposed, the muzzle end of the mould is on a level with the mouth of the pit, and the centre line of the gun is perpendicular.

The pit which receives the flask is twenty-eight and a half feet deep, and fourteen feet in diameter. Inside it, near the bottom, grate bars radiate like the spokes of a wheel. Underneath on the outside, these flues communicate a draft. Another flue, leading from the top of the pit, carries off the smoke. The object is to maintain a strong fire around the flask. This is one of the precautions in the process of casting guns.

The core is a long, hollow, cast iron, fluted barrel, hung vertically in the centre of the mould. It is exteriorly coated—firstly, by a coil of three-eighths inch rope, closely wrapped, and a mile in length ; and, secondly, with a luting of stiff clay, which is put on to separate the cast iron of the barrel from the molten iron of the gun. The rope is used to keep the clay out of the grooves, and, by presenting a rough surface, to prevent it slipping off. The grooves allow the gas (mainly hydrogen), formed by the contact of the melted metal and the clay, to escape freely at the upper end into the air, where it burns with quite a large flame. After the coatings are on, the core is likewise baked hard in an oven. Its outside diameter is nineteen inches, one inch less than the finished diameter of the bore, thus allowing half an inch all 'round for the completion of the latter.

The core barrel is kept cool by means of a stream of cold water circulating throughout its whole length. A small pipe extending almost to the bottom of the core is traversed by the water which, rising, fills the barrel, and passes off at the top. During the casting, the flow of water was sixty gallons per minute.

Three large reverberatory or air furnaces are used. Two are of the capacity of twenty-five tons each. The third, of the capacity of forty tons, is believed to be the largest in the world. It was built expressly for this casting. It was charged with thirty-nine tons, and each of the smaller furnaces was charged with twenty-three and a half tons. From these furnaces the iron is conducted through iron troughs, lined with clay, into a common pool near the pit. Thence it is conducted by two troughs into the gates of the mould. These gates are two openings, three inches in diameter, extending all the way down the mould outside the matrix of the gun. They communicate with the gun proper by means of smaller gates, cut through at intervals of fifteen inches, all the way up. The iron is thus conducted first into the bottom of the mould, and then through the side-gates, respectively, up to the top, as the metal rises in the mould. The difference between the reverberatory and the cupola furnace is that in the former, the fuel and iron are separate, and there is a natural draft through a large stack. In the latter, the fuel and iron are together, and the draft is made by blast.

Two cranes are used in lifting this heavy mould, and in taking the gun from the pit. They have each the capacity of forty tons. They are worked by the steam engine, which not only hoists and lowers the load, but likewise causes the cranes to revolve.

The metal from which the gun is manufactured is the Juniata iron, from the Bloomfield-Juniata and Rodman furnaces.

It has been once re-melted before being charged into furnaces for the gun. The iron from which it is constructed is thus all of one grade.

The furnaces were fired at five o'clock, A. M.; the metal was all melted by eleven o'clock. Whilst the iron was running in the mould, it was constantly stirred by men with long poles of oak. The object of this was to relieve it of the gas often formed in gun-irons, and which, if not removed, creates globular cavities in the mass of the gun, and thereby destroys its uniformity.

The casting commenced at twenty-four minutes after twelve. Eighteen bushels of coal were allowed to each ton of iron to make the fusion. Furnace number five was first opened, and then furnaces numbers six and four; four men with long, narrow puddling sticks, collected the scum and scoriæ which gathered on the surface of the liquid iron. Small quantities of iron where thrown into the pit from time to time, in order to light the gas which escaped from the sand through the small vent-holes in the flask.

The gas escaping from within the iron pipe surrounding the core barrel, was in a constant state of ignition, and formed a pale, luminous crown of blue flame; the last furnace was closed at forty-five and a half minutes past twelve, and the time occupied in the casting was twenty one and a half minutes.

The temperature of the stream of water was at the commencement, thirty-six degrees; at the moment the casting was over, it stood at forty-two degrees; four minutes after the casting was over it was at fifty-two degrees; eight minutes after the casting was over, at seventy-five and a half degrees; fourteen minutes after the casting, at eighty-one and a half degrees; twenty minutes after the casting, at eighty-nine and a quarter degrees; twenty-five minutes after the casting, at ninety-one degrees; and thirty minutes after the casting, at ninety-one and three-quarter degrees.

Nothing went wrong. Furnace No. 6 was the first to be stopped, and the others, Nos. 4 and 5, were stopped almost immediately after. When the casting was completed, a blacking, consisting of ground coal, was strewn over the top to prevent chilling. During the operation the gas escaped freely from the vent-holes in the flask, which is perforated in a thousand places, half an inch in diameter. Three furnaces only were employed. A fourth furnace was in reserve, in case of any accident, but there was no necessity for its use. Only two or three tons were left over after the casting was completed.

The fourth furnace in reserve had a supply of ten tons, and a fifth twelve tons, which in any exigency might have been drawn upon. After the casting was over, the mould, not being completely filled, owing to the gradual settling of the liquid mass, was supplied with fresh liquid iron, or, in other words, the workmen "filled up the shanks." All the fuel employed was the Pittsburgh bituminous coal.

The drawing of the core was to take place about twenty-four hours after the casting. In this operation the supply of water is suddenly stopped off. The barrel is much expanded by heat. A large strain is put on by the steam crane, almost sufficient to draw the barrel out on the re-admission of water. The barrel cools quickly, shrinks with rapidity, the strain of the crane springs it out about two feet, and it is then drawn out.

The buildings, cranes, furnaces, patterns, flask, and lathe, had to be entirely new-constructed in order to accommodate themselves to the size of the gun.

The lathe is sixty feet long, eight feet wide over the shears, and is driven by two engines, 6-inch cylinders, 12-inch stroke. In the foundation there are one hundred and twenty thousand bricks. The costly nature of the gun may be appreciated when it is remembered that the lathe which merely turns and finishes it cost between ten and fifteen thousand dollars.

The gun will be hung on trunnions placed over its centre of gravity, and allowing no preponderance. This mode of hanging guns, renders their manipulation in service much easier than when a great preponderance is at the breech.

The gun carriage does not deviate in its general construction from the wrought iron carriages of the 15-inch guns. The carriage is now being manufactured at the Watertown Arsenal, near Boston.

No statement can at present be given respecting the tenacity and hardness of the metal. It is to be remarked, that by the interior cooling principle adopted, the hardest and best iron is that nearest the surface of the bore. The fact of the gun being cast hollow, secures this.

The iron of which the gun is cast was thoroughly tested, in the same manner as that of the first 15-inch. The sample by which the gun is to be proved is to be taken from the sinking-head, or the rough end cut off from the muzzle.

Special cars or trucks are being built by the Pennsylvania Railroad Company for the proper conveyance of the gun. Its great weight concentrated in a small space might prove destructive to ordinary cars. A very large truck is therefore rendered necessary. The length of the gun is thus distributed.

The new proving ground belonging to the Fort Pitt Foundry, and known as Fort Pitt station, is seventeen miles out of the city, on the line of the Pennsylvania Railroad. A large traversing train, propelled by steam, has there been erected to handle the gun. Two heavy triangular frames, about forty feet high, support a movable beam on a steel knife-edge bearing, from which heavy rods descend and catch the trunnions and breech of the gun. Suspended in these rods the gun will be three feet from the ground, and after a firing will vibrate a pendulum between the frames. The shot discharged will be thrown into a steep but soft rock.

The powder used here is $\frac{13}{16}$ of an inch in diameter, or about the size of hickory-nuts. The gun will burn not less than one hundred pounds of powder.

The 20-inch is not intended for a long-range gun, but rather to do execution within the range which it will possess. In the manufacture of guns, of large calibre, the question which arises is, "to which shall the greatest attention be devoted, to the increase of velocity, or to the bulk of the cannon ball ?" The weight of the shot has in this instance engrossed consideration. Shell will be almost exclusively used. Both shot and shell will be spherical, of great weight and enormous explosive force. The solid shot will weigh one thousand pounds, and the shell seven hundred and seventy-five pounds, allowing for windage. No information of course can be given at present in regard to the range of the gun.

The value is estimated at thirty thousand dollars. The gun is expected to be ready in the latter part of May. The expense for each charge of the gun may be roughly placed at about seventy-five dollars, allowing twenty-five dollars for powder, and fifty for projectiles. Fifteen or sixteen men will be required to manage the gun when completed, nine being employed in the loading, and six or seven in traversing the gun.

The casting was then taken from the pit on the 25th of February, having been permitted gradually to cool until that time, and was then found apparently perfect.

Oxygen Gas.

From the London Chemical News, No. 212.

Dr. Frankland stated that he had lately been engaged in some experiments wherein he was desirous of using oxygen gas under great pressure, and for this purpose had procured the apparatus for the compression of gases made by Natterer, of Vienna. During the employment of this apparatus an accident happened, which fortunately occasioned no serious disaster, but was the means of indicating a result which was not likely to have been anticipated, and which he considered well worthy the attention of the Society. The apparatus consisted of two parts. On the one hand, a powerful compression pump, worked by a crank and fly-wheel, which forced the oxygen or other gas into a strong cast iron receiver, fitted with a conical screw-plug and other joints and connexions of steel. The action of the pump had been maintained until twenty-five atmospheres of oxygen had been accumulated within the receiver, when suddenly this latter part of the apparatus exploded, at the same time diffusing a shower of sparks, reminding one of the phenomenon observed when iron or watch-spring was burnt in oxygen. On examining the shattered fragments of the iron receiver, it was manifest that this result had indeed happened ; and it appeared that the combustion of iron was possible under the circumstances, a small quantity of oil used as a lubricant becoming, no doubt, in the first instance, ignited, and then imparting the combustion to the iron. The whole interior surface of the iron receiver was blistered and coated with fused globules of magnetic oxide of iron, the small tubular apertures through the screw-plug were widened to four-fold dimensions, and no less than half an inch of steel was burnt off the massive head of the apparatus. It was, perhaps, so far fortunate, that a defective joint rendered it impossible to obtain more than the pressure of twenty-five atmospheres, under which the apparatus exploded, for had it been augmented to forty atmospheres, the iron must have taken fire more readily, and perhaps have been completely consumed or hurled about as red-hot bolts, endangering both life and property. The author had, by a rough calculation, arrived at the conclusion, that the heat developed by the union of half the contained oxygen with the iron, would have been sufficient to melt the cylinder, which would then have exploded by the remaining pressure of the other half of the gas. Dr. Frankland concluded by referring to the possibility of applying this principle in the construction of shells and other implements of warfare.

In answer to an inquiry on the part of the President, Dr. Frankland stated that the degree of pressure in the apparatus was estimated, not by a gauge, but by actual measurement of the oxygen gas passing in at the ordinary temperature and pressure from a pneumatic holder. It was right also to mention the fact of water and potash being likewise contained in the iron reservoir at the moment of the explosion.

Dr. W. A. Miller once met with an accident in his laboratory at King's College, which, although totally different in character from that just before narrated by Dr. Frankland, should be made known by way of warning chemists from attempting a like experiment. The speaker had been using a powerful Rühmkorff coil for the purpose of effecting the combination of the mixed oxygen and hydrogen gases in Cavendish's eudiometre, and when the first charge had thus been disposed of, the stop-cocks were opened for the admission of a fresh supply of the mixed gases. At this moment, whilst yet disconnected with the electric coil, the whole arrangement exploded, and, so far as the instantaneous character of the phenomenon could be observed, it appeared to be due to the formation of a fine deposit of spongy platinum upon the metallic wires inserted into the eudiometre, the result of the employment of an electric instrument of too great intensity. By reverting to the old method of causing the ignition by the spark from a Leyden jar, no difficulty or danger had ever been experienced.

A Substitute for Wood Engraving.

From the London Art Journal, Nov., 1863.

Multifarious have been the attempts, throughout the latter ages of the world especially, to bring scientific knowledge to bear upon the productions of human industry, in order to lessen, if not entirely to supersede, the efforts of manual labor; and so far as the operations have been directed to the machinery of construction and to manufacturing processes, the results have proved marvellous. It seems as if no limit could be assigned to the ingenuity of man in devising and creating what would best minister to his absolute necessities in all instances, and to his gratifications in not a few. Similar successes have not, however, followed his endeavors when he has ventured into the region of Art; here matter will not yield obedience to mind, so to speak; though photography and chromo-lithography, and other methods of artistic production, have done something in the way of superseding the hand-work of the painter, and machinery is employed to do that which, a few years ago, was accomplished only by the tools of the sculptor. Art, as a creator and skilled workman, defies all scientific aid; it is, and must be, self-dependent.

Perhaps there has been no other department of Art in which so many efforts have been made to find a substitute for actual labor as wood-engraving; hitherto, however, all have failed, and the engravers are still left in quiet possession of the field; nor are we by any means sure that what we now lay before our reader will be the means, even ultimately, of dislodging them from the position they have so long and so honorably held in the domain of Art. But it is our business to inquire into whatever comes before us in the way of novelty which bears on the face of it a probability of success, while it is a duty to any one who seems to have made out a case to give it all the publicity in our power. For this purpose we introduce an account of Mr. Schulze's process of producing blocks for type-printing, intended to do away with wood engraving.

Mr. Schulze is a German architect, resident in New York, but now staying in London for a short time. He informs us that the material on which the drawing is to be made may be of glass, or any other hard and smooth surface. The drawing is produced with a pen, and ink composed of pure gum arabic dissolved in water, with the addition of sufficient sugar to prevent it cracking when dry; lamp-black, or any other color, is mixed with the gum solution to render the work visible. When the drawing is completed, it is covered with a coat of bees-wax, asphaltum, resin, and linseed oil. The thickness of the covering depends on the kind of work adopted by the artist; if the lines of the drawing are very close together, a thin coat will suffice. After this ground has been applied, the plate, or glass, has to be submerged in water for about ten or fifteen minutes; then a strong stream of water is poured upon it, which will remove the waxy substance *above* the lines of the drawing, but will leave that *between* the lines undisturbed. In most cases the grounding will be sufficiently high to insure a good electrotype for printing; but where considerable height is required between lines far apart, this can readily be effected by applying wax according to the method now employed by stereotypists, or by adding asphaltum with the brush. Should the artist prefer to make his drawing on paper, the latter must be rendered waterproof; and after it has undergone this process, it should be attached, with a waterproof paste, to a hard and even plate before it is covered with the wax; in all other respects it is treated in the manner just described. Before taking the electro deposit, the plate must be covered with a thin coat of alcoholic varnish, and when dry, black lead—plumbago—is applied with a soft brush.

The advantages of the process are stated by the inventor to be:—The obtaining a perfect fac-simile of the artist's work; the drawing has not to be reversed, as in the methods now in use for copying on the wood pictures or objects; cheapness, and saving of time.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, January 21, 1864.

John C. Cresson, President, in the chair.

John F. Frazer, Treasurer.

Robert Briggs, Recording Secretary, pro tem.

The minutes of the last meeting were read and approved.

A letter was read from Messrs. Eives and Macey, London.

Donations to the Library were received from the Royal Astronomical Society, the Institute of Actuaries, and the Society of Arts, London; de Oesterriechischen Ingenieur-Veriens, Vienna, Austria; Professor E. M. Dingler, Augsburg, Germany; the Natural History Society, and Major L. A. Huguet-Latour, Montreal, and the University of Toronto, Canada; the Hon. Commissioner of Agriculture, the Hon. Wm. D. Kelley, Prof. A. Dallas Bache, and Frederick Emmerick, Esq., Washington, D. C.; the Hon. Mr. Brady, Senate, Harrisburg, Penna.; and Prof. John F. Frazer, Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer read his statement for December, 1863, also his annual statement for the year 1863.

The annual statement of the Committee on Publications on the state of the Journal of the Institute, for 1863, was read.

The Board of Managers and Standing Committees reported their minutes.

Candidates for membership in the Institute (16) were proposed, and the candidates proposed at the last meeting (12) were duly elected.

The Tellers of the Annual Election for Officers, Managers, and Auditors, for the ensuing year, reported the result, when the President declared the following gentlemen duly elected:—

William Sellers, President.

John H. Towne, } Vice Presidents.
Fairman Rogers, }

Washington Jones, Recording Secretary.

Robert Briggs, Corresponding Secretary.

John F. Frazer, Treasurer.

MANAGERS.

William Adamson,
B. Henry Bartol,
Charles H. Cramp,
Pliny E. Chase,
Charles S. Close,
William Divine,
James Dougherty,
George Erety,
John Gardiner, Jr.
George Harding,
James Hunter,
W. Barnet Le Van,
J. Vaughan Merrick,
Wm. A. Mitchell,
Bloomfield H. Moore,
Henry G. Morris,
Charles T. Parry,
Percival Roberts,
Samuel J. Reeves,
Coleman Sellers,
Samuel Sloan;
Samuel S. White,
S. Lloyd Wiegand,
O. Howard Wilson.

AUDITORS.

Samuel Mason,
James H. Cresson,
William Biddle.

At a meeting of the Board of Managers, held January 27th, 1864, the following officers were elected for the ensuing year :—

James Dougherty, Chairman.
Bloomfield H. Moore, } Curators.
W. Barnet Le Van, }

On motion of Coleman Sellers, the thanks of the Franklin Institute was tendered to Professor Cresson for the able and efficient manner in which he has filled the office of President, the past *nine* years.

Mr. Briggs remarked that is was eminently proper, that upon retiring from the office, as Professor Cresson does this evening, the thanks of the Institute be offered him individually, as a mark of the appreciation of the ability and attention with which he had for so many years so honorably filled the highest seat in its councils.

The Institute represented not only the relation between the mechanic and the scientific man in Philadelphia, but was beyond that, the representative institution of the connexion of mechanics with science for the country. No similar institution had its reputation either at home or abroad, and that position has been reached by those who cotemporaneously with Professor Cresson gave it an original direction, and it has been supported in that position the last nine years, under his leadership. To no one more than to our retiring President, do we owe the elevation of the Institute, and on this occasion, when new and inexperienced hands have been entrusted with its direction, we can only mark our estimation of his example by a vote of thanks.

Mr. Cresson replied, that he highly appreciated the personal kindness that prompted the gentleman to move this resolution, and the kindness of which its adoption by the Institute gave evidence. He must, however, disclaim the credit of having contributed in any large degree to the honorable position of the Institute as a pioneer in the noble work of raising the labor of the mechanic and artisan to its true position as both handmaid and exemplar of science. He was not one of the original members or founders of the Institute, having been absent from his native city during the years in which it had its inception; but united himself with it immediately upon his return to the city, and has for more than twenty-five years been happy to take a share in its labors.

For ten years he filled the chair of Mechanics and Natural Philosophy, in the lecture room; for about twenty years has acted as chairman of the Committee on Science and the Arts; and during the last nine years has been honored with the office of President. For these repeated marks of confidence, he is happy to express his grateful appreciation; and trusts that the Institution under the impulse of the younger and more active members who are now to guide its destiny, will put forth its strength in a long career of increasing usefulness.

On motion of Prof. Cresson, the thanks of the Institute were tendered to the retiring Vice Presidents, Secretaries and Managers for the able support and direction of the Institute during many years of official connexion.

Mr. P. E. Chase gave an abstract of his recent communications to the American Philosophical Society, on the barometer, as an indicator of the earth's rotation, and the sun's distance.

The existence of daily barometric tides, has been known for more than a hundred and fifty years, but their cause is still a matter of dispute. It is evident that they cannot be accounted for by variations of temperature, for, 1, their regularity is not perceived until all the *known* effects of temperature have been eliminated; 2, they occur in all climates, and at all seasons; 3, opposite effects are produced at different times, under the same average temperature. Thus at St. Helena, the mean of three years' hourly observations, gives the following average barometric heights:

From 0h. to 12h.	28·2801 in.	From 18h. to 6h.	28·2838 in.
" 12h. to 0h.	28·2861 "	" 6h. to 18h.	28·2784 in.

The upper lines evidently embrace the coolest parts of the day, and the lower lines the warmest. Dividing the day in the first method, the barometer is highest when the thermometer is highest; but in the second division the high barometer prevails during the coolest half of the day.

On account of the combined effects of the earth's rotation and revolution, each particle of air has a velocity in the direction of its orbit, varying at the equator, from about 65,000 miles per hour, at noon, to 67,000 miles per hour, at midnight. The force of rotation may be readily compared with that of gravity, by observing the effects produced by each in twenty-four hours, the interval that elapses between two successive returns of any point, to the same relative position with the sun. The force of rotation producing a daily motion of 24,895 miles, and the force of terrestrial gravity a motion of 22,738,900 miles, the ratio of the former to the latter is $\frac{24895}{22738900}$, or ·00109. This ratio represents the proportionate elevation or depression of the barometer above or below its mean height, that should be caused by the earth's rotation, and it corresponds very nearly with the actual disturbance at stations near the equator.

From 0h. to 6h. the air has a forward motion greater than that of the earth, so that it tends to fly away; its pressure is therefore diminished, and the mercury falls. From 6h. to 12h. the earth's motion is greatest; it therefore presses against the lagging air, and the barometer rises. From 12h. to 18h. the earth moves away from the air, and the barometer falls; while from 18h. to 24h. the increasing velocity of the air urges it against the earth, and the barometer rises.

If the force of rotation at each instant be resolved into two components, one in the direction of the radius vector, and the other parallel to the earth's orbit, it will be readily perceived that whenever the latter tends to increase the aerial pressure, the former tends to diminish it, and vice versa. Let B= the height of the barometer at any given instant; M= the mean height at the place of observation; $\theta - 90° =$ the hour angle; C= the earth's circumference at the equator; t=24 hours; g= the terrestrial gravity; l= the latitude; and a simple integration gives the theoretical formula, $B = M\left(1 + \frac{\sin.\ \theta.\ \cos.\ \theta.\ \cos.\ l}{R^3}.\ \ \frac{2C}{gt^2}\right)$*.

This formula gives a maximum height at 9h. and 21h. and a minimum at 3h. and 15h. The St. Helena observations place the maximum at 10h. and 22h., and the minimum at 4h. and 16h.—an hour later in each instance, than the theoretical time. This is the precise amount of retardation caused by the inertia of the mercury, as indicated by the comparisons with the water barometer of the Royal Society of London.

Aerial currents, variations of temperature, moisture, and centrifugal force, solar and lunar attraction, the obliquity of the ecliptic, and various other disturbing causes, produce, as might be naturally expected, great differences between the results of theory and observation. But by taking the grand mean of a series of observations, sufficiently extended to balance and eliminate the principal opposing inequalities, the two results present a wonderful coincidence.

According to our formula, the differences of altitude at 1, 2, and 3 hours from the mean, should be in the respective ratios of ·5, ·866, and 1. The actual differences, according to the mean of the St. Helena observations, are as follows.—

	Differences of Barometer.			Ratios.		
Diff. of time	1h.	2h.	3h.	1h.	2h.	3h.
Before 1h,	·0166	·0298	·0365	·455	·816	1
After "	·0159	·0266	·0298	·534	·893	1
Before 7h.	·0122	·0202	·0243	·502	·831	1
After "	·0135	·0239	·0297	·455	·805	1
Before 13h.	·0136	·0248	·0284	·479	·873	1
After "	·0131	·0215	·0227	·577	·947	1
Before 19h.	.0161	·0287	·0348	·463	·825	1
After "	·0150	·0265	·0286	·524	·927	1
MEAN	·0145	·0252	·0293	·495	·860	1

The mean of the above differences, varies from the theoretical mean, less than $\frac{1}{5000}$ of an inch. If we take the mean of the ratios, instead of the ratios of the means of the observed differences, the coincidence is still more striking.

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* $\frac{C}{gt^2}$ represents the effective ratio of an entire day. But there is in each day a half day of acceleration, and a half day of retardation, and the ratio for each half day is $\frac{C}{2} \div \frac{gt^2}{4} = \frac{2C}{gt^2}$.

Difference of Time,	1h.	2h.	3h.
Means of Observed ratios,	·498625	·864625	1·000000
Theoretical Means,	·500000	·866025	1·000000

The calculated time for the above observed means, differs less than 20″ from the actual time.

Observed Means,	·498625	·864625	1·000000
Theoret. Diff. of Time,	59′ 48″	119′ 40″	180′
Observed " " "	60′ 0″	120′ 0″	180′

The varying centrifugal force to which the earth is subjected by the ellipticity of its orbit, must, in like manner, produce annual tides. The disturbing elements render it impossible to determine the average monthly height of the barometer with any degree of accuracy, from any observations that have hitherto been made. We may, however, make an interesting approximation to the annual range,—still using the St. Helena records, which are the most complete that have yet been published for any station near the equator. Comparing the mean daily range, as determined by the average of the observations at each hour, with the mean yearly range, as determined by the monthly averages, we obtain the following results:

Year.	Daily Range.	Annual Range.	Ratio.	Approximate Solar Distance.
1844	·0672 in.	·1650 in.	2·4553	137,070,000 m.
1845	·0646 "	·1214 "	1·8793	80,300,000 "
1846	·0670 "	·1214 "	1·8120	74,650,000 "
	3)·1988	3)·4078	3)6·1466	
	·0663	·1359	2·0489	95,446,000 "
Mean	·0663	·1290	1·9457	86,056,000 "
	2)·1326	2)·2649	2)3·9946	
	·0663	·1324	1·9973	90,702,000 "

The approximate estimates of the solar distance, are based on the following hypothesis:

Let e = effective ratio of daily rotation to gravity.

a = arc described by force of rotation in a given time t.

r = radius of relative sphere of attraction, or distance through which a body would fall by gravity, during the disturbance of its equilibrium by rotation.

A = area described by radius vector in time t.

Let e', a', r', A′, represent corresponding elements of the annual revolution. Then,

$$\text{A} : \text{A}' : : ar : a' r' : : e^2 : e'^2$$

But the forces of rotation and revolution are so connected, that a differs but slightly from a'

$$\therefore \quad \left.\begin{array}{l} e^2 : e'^2 : : r : r' \\ r' = \dfrac{e'^2 r}{e^2} \end{array}\right\} \text{very nearly.}$$

It may be interesting to observe how nearly r (22,738,900 m.) corresponds with Kirkwood's value of $\frac{D}{2}$ (24,932,000 m.). A more thorough comprehension of all the various effects of gravity and rotation on the atmosphere, would probably lead to modifications of our formulæ that would show a still closer correspondence.

There is a great discrepancy between the determinations of the solar distance that are based on the records of 1844 and 1846, but it is no greater than we might reasonably have anticipated. On the other hand, it could hardly have been expected that any comparisons based on the observations of so short a period as three years, would have furnished so near an approximation to the most recent and most accurate determination of the earth's mean radius vector. In order to obtain that approximation, it will be seen that I took, 1st, the mean of the ranges and ratios for the three successive years; 2d, the ranges and ratios of the mean results of the three years; 3d, the grand mean of these two primary means. I could think of no other method which would be so likely to destroy the effects of changing seasons, and other accidental disturbances.

The following table exhibits the effects of latitude on the aerobaric tides. The differences between the theoretical and observed ranges, may be owing partly to the equatorial-polar currents, and partly to insufficient observations.

Station.	Lat.	Mean height.	Mean Range.	Ratio.	Theoret. Ratio.
Arctic Ocean,	78°37′	29·739 in.	·012 in.	·000404	·000527
Girard College,	39 58	29·938	·060	·002004	·002046
Washington,	38 53	30·020	·062	·002065	·002079
St. Helena,	15 57	28·282	·066	·002344	·002567
Equator,	0	30·709	·082	·002670	·002670

The theoretical ratios are determined by multiplying the equatorial ratios by $\frac{\cos l}{R}$. The formula, $\rho = \frac{\cos l}{R} \cdot \frac{2c}{gt^2}$, ($\rho$ indicating the ratio of the mean range to the mean height,) gives

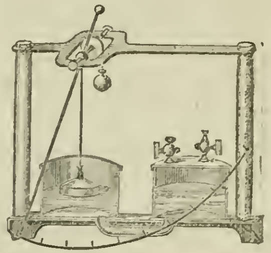
		Theoretical Ratio.	Observed Ratio.
Latitude,	0°	·002190	·002670
"	78 37′	·000432	·000404

showing that the ratio is less near the pole and greater near the equator, than our theory indicates, a natural consequence of the centrifugal force at the equator, and the cold surface currents that produce the trade winds.

The revolution of the sun around the great Central Sun must also cause barometric fluctuations that may possibly be measured by delicate instruments and long and patient observation. The Toricellian column may thus become a valuable auxiliary in verifying or rectifying our estimates of the distances and masses of the principal heavenly bodies.

Mr. Robert Briggs called the attention of the meeting to an instrument for measuring small pressures of air, such for example as were formed in ventilating air ducts, either when fans were used for impelling the air, or when self-acting currents were produced by heat; or in short, the amount of pressure which any current of air might possess. This instrument consisted of two cylindrical cups made of exactly the same diameter, the size being immaterial, but the exact correspondence of diameter was readily produced by the workmen using the same length of sheet metal in forming the two cylinders. One of these cups is closed except a cock to receive the pressure of air through a flexible tube which shall connect the instrument with the duct in which the pressure is to be measured; an air cock for setting the index, and a tubular connexion at the bottom with the other cup.

The other cup, connected to the first by a tube in the bottom, is open to the air. The open cup has a float depending from a cord which passes over a small grooved pulley, and terminates in a weight. The pulley is fast upon the shaft which carries the hand. This shaft is very lightly hung on centres, and the hand is carefully balanced, so that the friction of the cord on the pulley will be sufficient to move the hand. Attached to the side of the cups is a dial segment against which the extremity of the hand marks the pressure, in the instrument drawn in the cut. The cups were three inches diameter and two inches deep; the hand was eight inches long and the quadrant ten inches on the curve. The ten inches of motion indicating one inch of water pressure.



To operate the instrument the cups are filled with water three-quarters of an inch to an inch deep, and the end of the finger placed at zero by sliding the cord on the pulley when the pressure cock is closed and the air cock open. By opening the pressure cock and closing the air cock, the water is depressed in the first cup and elevated in the second, raising the float and thus transferring the movement greatly increased to the hand. By agitating the instrument so as to wet the sides of both cups before making the trial, correct indications of one-hundredth of an inch water column are readily attained. In order to obtain the mark on the quadrant originally without calculating the exact proportion of pulley to length of hand, the thickness of cord, &c., it is only necessary to thrust a needle through the cord, and then after setting the hand at zero, produce such a pressure as will move the needle (the point of which can rest on a slip of card) a half inch; the movement of the hand on the quadrant then indicates one inch of difference of level in the two cups, and the extreme length thus ascertained can be divided to any desirable extent. For more feeble pressures the ratio of radius of pulley to length of hand can be increased by reduction of pulley, thus producing more motion of extremity of hand for a given pressure. There are similar instruments for considerable pressures, say one to six inch water column, but the speaker thought none, not requiring nice calculations as to areas of float chamber compared with water chamber, as well as the adjustment of pulley and length of hand, all of which ended in comparison with other instruments, for a basis of pressure measure, but he had not met with one before, which could be adjusted by itself, and was equally portable.

Professor Cresson described a Pressure Indicator used at the City Gas Works, for showing the pressure of the gas in various parts of the factory, and in the street mains, with all of which it is connected by small tubes provided with stop-cocks, that allow any one main to be in communication with the instrument, while all the others are cut off. These connecting tubes open into the upper part of a gas-tight receiver kept partly filled with water, or other liquid. An open stand pipe of small sectional area in comparison with the area of the receiver, passes through the top of the instrument, and descends low enough to have its lower end always immersed in the liquid, and thus sealed against all escape of the gas which occupies the upper portion of the receiver. The change of level of the liquid column in the stand pipe gives motion to a float, and this is communicated to a revolving index by a weighted cord running over a small pulley fixed on the arbor of the index.

The indications of pressure by the rise of the liquid in the stand pipe, are magnified in a ratio determined by the relative diameters of the small pulley that moves the index and of the graduated arc traversed by the latter.

If great accuracy is required, the minute error arising from the small changes of level of the liquid in the receiver as portions of it pass into, and out of, the small stand pipe, may be corrected in the divisions upon the graduated arc. For example, if the relative area of the receiver and the dip pipe, be 50 to 1, then the divisions should be one-fiftieth smaller than their designation indicates.

A stop-cock placed on the top of the receiver to admit atmospheric pressure, allowed the instrument to be brought to zero after each experiment.

Prof. Cresson also gave an account of experiments upon the momentum of air or gas flowing in closed pipes of large diameter. In a 20-inch gas main three miles long, he had found the momentum of the column to produce an excess of pressure, on closing a valve at one extremity, equal to a six-inch column of water, and the successive reflections of the wave to create nearly this excess of pressure alternately at the opposite ends of the pipes for many successive oscillations of pressure in the gaseous column.

Mr. Skerving exhibited a large Engraving by John Sartain, (from the original painting by C. Schussele,) entitled "Men of Progress." It represents a group of nineteen of the prominent American inventors and discoverers.

The names of the artists are sufficient guarantees of the excellence of the work, which is a well deserved compliment to those whose exertions have contributed so much towards the advancement of science and the arts.

Mr. J. T. Kerby exhibited a patent Horse-shoe. This is said to be the only shoe in use which is beveled on the inside so as to permit the dirt to readily detach itself from the hoof. There are seven instead of three "calks," as in the ordinary shoe, and consequently that even bearing all around the wall or crust, which the old shoe takes away, is secured.

Mr. Hiram A. Kimball exhibited his patented Artificial Limbs, made of vulcanized india rubber. They are light in weight, well shaped, and exceedingly strong. Being hollow, all the machinery is contained inside, and is not liable to be deranged or broken. These limbs can be much more readily manufactured, and in less time, than those carved from wood, or made of iron, as in the ordinary manner.

Mr. Thomas Shaw sent to the meeting, as a donation to the Institute, a very handsome figure of an individual standing on a trunk, and exerting all his strength in endeavoring to raise himself and the lid on which he was standing by pulling at the handles attached to it.

In the note, accompanying the figure, Mr. Shaw says—"I determined upon giving a short spicy illustration of a whole lecture on the subject, and to illustrate the folly of ever attempting to overcome that one *dead point* which attends all perpetual motion schemes."

A number of specimens of Wood Graining, executed by Anthony Goth, of Bethlehem, Pa., was exhibited by Mr. Jones.

The appearance of the natural grain in these specimens, is so admirably imitated as to almost defy detection, and affords ample testimony to the truth of the inventor's assertion, that the process is the most perfect yet imitated. Mr. G. has given no information as to what the process is, there not having been sufficient time before the exhibition night to prepare the tools.

ERRATA.

In S. W. Robinson's article on *Suspension Bridges*, in the number for September, 1863, page 146, under the radical of second equation preceding equation (1), there should be — $2TT \cos a$, in lieu of — $TT \cos a$.

Page 147, the first term of equation (9) should be $\frac{5}{2}$ power, instead of $\frac{3}{2}$.

Page 149 equation (17) should be—

$$\left[1+\Bigl(\frac{x}{2y}\pm\sqrt{\left(\frac{x}{2y}\right)^2-2}\;\right]^{\frac{3}{2}}=\left[\frac{x}{2y}\pm\sqrt{\left(\frac{x}{2y}\right)^2-2}\;\right]^2\frac{\rho_0}{2y}.$$

A Comparison of some of the Meteorological Phenomena of JAN., 1864, *with those of* JAN., 1863, *and of the same month for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	January, 1864.	January, 1863.	January, for 13 years.
Thermometer—Highest—degree, .	65·00°	61·00	65·00°
“ “ date, .	29th.	15th.	29th, 1864.
“ Warmest day—Mean,	52·50	58 33	58·33
“ “ “ date,	28th.	15th.	15th, 1863.
“ Lowest—degree, .	7·00	14·00	—5·50
“ “ date, .	2d.	18th.	23d, 1857.
“ Coldest day—Mean,	12·00	20·67	—1·00
“ “ “ date, .	2d.	18th.	9th, 1856.
“ Mean daily oscillation,	13·74	12·90	11·94
“ “ “ range, .	5·85	5·60	6·48
“ Means at 7 A. M., .	29·89	33·95	28·16
“ “ 2 P. M., .	37·48	40·95	35·68
“ “ 9 P. M., .	32·24	36·64	31·46
“ “ for the month,	33·20	37·18	31·77
Barometer—Highest—Inches, .	30·271 in.	30·571 in.	30·704 in.
“ “ date, . .	7th.	18th.	28th, 1853.
“ Greatest mean daily press.	30·207	30·553	30·609
“ “ date, . .	21st.	18th.	28th, 1853.
“ Lowest—Inches, .	29·257	29·127	28·911
“ “ date, . .	19th.	16th.	23d, 1853.
“ Least mean daily press.,	29·347	29·298	29·086
“ “ date, . .	19th.	16th.	23d, 1853.
“ Mean daily range, .	0·206	0·266	0·216
“ Means at 7 A. M., .	29·886	29·925	29·965
“ “ 2 P. M., .	29·847	29·867	29·924
“ “ 9 P. M., .	29·913	29·905	29·954
“ “ for the month, .	29·882	29·899	29·948
Force of Vapor—Greatest—Inches,	0·396 in.	0·462 in.	0·505 in.
“ “ “ date, .	1st.	16th.	11th, 1858.
“ “ Least—Inches, .	·025 in.	·057	·023
“ “ “ date, .	2d.	18th.	22d, 1857.
“ “ Means at 7 A. M.,	·133	·166	·136
“ “ “ 2 P. M.,	·137	·175	·153
“ “ “ 9 P. M.,	·140	·172	·147
“ “ “ for the month,	·137	·171	·145
Relative Humidity—Greatest—per ct.,	100 per ct.	100 per ct.	100 per ct.
“ “ “ date,	18th.	21st.	Often.
“ “ Least—per ct.,	34·0	38	24·0
“ “ “ date, .	26th & 28th.	2d & 3d.	25th, 1860.
“ “ Means at 7 A.M.,	73·1	78·0	79·7
“ “ “ 2 P.M.,	57·8	65·3	68·2
“ “ “ 9 P.M.,	69·5	74·8	76·4
“ “ “for the month	66·8	72·7	74·8
Clouds—Number of clear days,* .	13	7	8·9
“ “ cloudy days, .	18	24	22·1
“ Means of sky cov'd at 7 A.M.,	60·0 per ct.	68·1 per ct.	63·3 per ct.
“ “ “ “ 2 P.M.,	52·9	70·0	62·8
“ “ “ “ 9 P.M.,	41·6	60·6	48·6
“ “ “ for the month,	51·5	66·2	58·2
Rain and melted Snow—Amount, .	1·878 in.	4·698 in.	3·238 in.
No. of days on which Rain or Snow fell,	10	14	10·7
Prevailing Winds—Times in 1000,	N78°41′W·319	N50°26′W·175	N.61°0′W·318

* Less than one-third covered at the hours of observation.

JOURNAL

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FOR THE

PROMOTION OF THE MECHANIC ARTS.

MARCH, 1864.

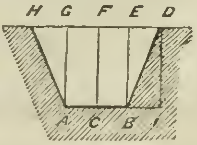
CIVIL ENGINEERING.

Railroad Cuttings and Embankments.—Side Depths and Side Stakes. By Oliver Byrne, C.E.

From the Lond. Civ. Eng. and Arch. Jour., Jan., 1864.

When the centre stumps of a railroad have been put down, which are usually at the distance of one chain, the line must next be leveled, and the number of stumps entered in the level-book in a vertical column; and opposite each number, in another column, the depth of the cuttings or embankments; and in a third column, the horizontal half-width of the surface cuttings. But every engineer has his peculiar method of keeping a field or level-book.

Fig. 1.



To set out the width of cuttings, when the surface of the ground is laterally level, and at a given height above the level of the intended railroad, the ratio of the slopes being given, let ABDH, Fig. 1, be the cross-section of a cutting, the ground HD parallel to the bed of the road AB; put CF, the height in the centre of the road $= h$, and the breadth of the road-bed AB $= b$; the slope of the side AH or BD is generally expressed by the ratio of the base BI to the perpendicular ID; let BI : ID $= m : n$, then the slopes are said to be $m : n$, or $\frac{m}{n}$.

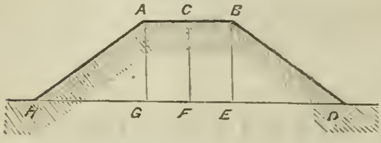
In this case, as it is supposed the ground HD is level, the distance from the centre F to the side stakes D and H will be expressed by

$$\tfrac{1}{2}b+\frac{m}{n}h.$$

Example.—Let the bottom width AB = 28 feet, the depth of the cutting CF = 16 feet, the slopes 5 : 4; that is BI : ID = 5 : 4; required the distance of the side stakes H and D from the centre F.

$$HF=\tfrac{1}{2}\text{ of }28+\tfrac{5}{4}\text{ of }16=34=FD.$$

Fig. 2.



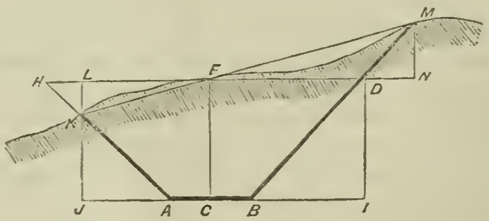
Example of embankment, when surface of the ground is laterally level (Fig. 2). Suppose the breadth of the roadway AB = 32 feet; the height of the embankment CF = 20 feet, and the ratio of the slopes 6 to 5, that is, DE : EB = 6 : 5; required the distance of the side stakes H and D from the centre F.

$$HF=\tfrac{1}{2}\text{ of }32+\tfrac{6}{5}\text{ of }20=40=FD.$$

The slope given to the sides of either cuttings or embankments varies with the material through which the road has to pass.

When BI = ID (Fig. 1), the slope is said to be 1 : 1. The slope is said to be a rise of 2 to 1 when IB = twice ID. In close-jointed rock the ratio varies from 1 : 4, to 1 : 2. In soft or loose-jointed rock, or stiff clay, the ratio varies from 1 : 1, to 3 : 2. If the road passes through moist springy ground or loose sand, the ratio of the slope varies from 2 : 1, to 5 : 2. Should the ground rise from F to M (Fig. 3),

Fig. 3.



the slope stake must be set out further, as at M. Let the additional height MN = p, then

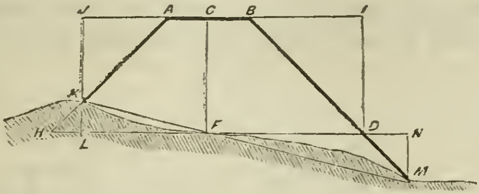
DN = $\frac{m}{n}p$. And if the ground falls from F to K, a distance LK = q, the slope stake must be set further in at K, a distance HL = $\frac{m}{n}q$.

$$\therefore\ FN=\tfrac{1}{2}b+\frac{m}{n}h+\frac{m}{n}p$$

$$FL=\tfrac{1}{2}b+\frac{m}{n}h-\frac{m}{n}q.$$

It often happens that p and q are unequal. Setting slope stakes for embankments resembles setting them for excavations, only a rise from the centre F with excavations, corresponds to a fall with embankments; in fact, an embankment is a cutting inverted. Fig. 4 represents an embankment, and is Fig. 3 inverted. The rise at K is nearer the centre F, than H on the level, in an embankment (Fig. 4), while the fall at K is nearer the centre F than H on the level with F in an excavation, (Fig. 3.)

Fig. 4.



Example.—In the cutting, Fig. 3, and in the embankment, Fig. 4,

Let AB $= b = 30$ feet. KL $= q = 4$ feet.
CF $= h = 18$ " $m : n = 3 : 2$.
MN $= p = 6$ "

$$3 : 2 = \text{BI} : \text{ID} = \text{AI} : \text{JK} = \text{DN} : \text{NM} = \text{HL} : \text{LK}.$$

Consequently

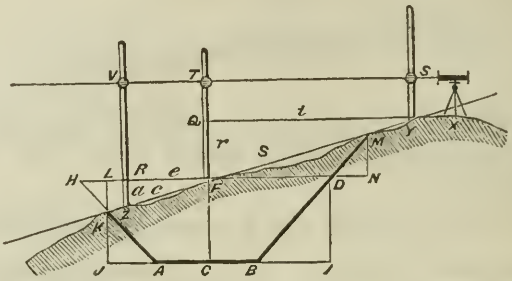
$$\text{FN} = \frac{30}{2} + \frac{3\times 18}{2} + \frac{3\times 6}{2} = 51 \text{ feet.}$$

$$\text{FL} = \frac{30}{2} + \frac{3\times 18}{2} - \frac{3\times 4}{2} = 36 \text{ feet.}$$

As the ground continues to slope up or down from F, the centre stake, the positions of M and K are often determined on the ground by a series of trials, or fudged out in an office by some clumsy mechanical construction or other. To avoid guessing, or rule-of thumb operations, I will lay down a practical exact plan by which the positions of the side stakes K and M may be easily found. The positions of D and H on a level with F, the centre stake, can be accurately calculated when the height CF, breadth AB, and ratio of BI to ID are given, therefore it is known, very nearly, where the lines AK and BM strike the surface of the earth. And as FK and FM are seldom in the same straight line, it is more accurate to find the ratio of FL to LK as well as the ratio of FN to NM (Fig. 5). When these ratios are known, which may be readily found by a level and target rod, the distance of M from F and of K from F are easily calculated.

In the neighborhood of M and K, there are always short spaces before or behind M and K, in the direction of the lines KF and FM, and it does not matter how irregular the surface is between these points. It is not necessary that the spaces before or behind K and M are level or not, so that they are nearly in the directions of FM and FK. Set up and adjust the level at any convenient place X, outside the cross section; place the target-staff at Y, as near as you are able to judge, to the required point M: read off the height SY; remove the staff to the centre stake F, and read off the height FT; the difference between SY and FT will give QF, which put $=r$, measure YF, and put $s=$ YF. If the distance QY be not great, it may be measured by the same tape or chain that takes the length of FY; or QY may be calculated, for QY $=\surd(s^2-r^2)$, which put $=t$ to make the reasoning more concise.

Fig. 5.



Then the three sides of the triangle QFY become known; this triangle is similar to the triangle FNM, and hence

$$\text{FN} : \text{NM} : : t : r$$
$$\text{FN} : \text{FM} : : t : s$$
$$\text{FM} : \text{MN} : : s : r$$

because the three sides of the triangle, YF, QF, QY, are respectively represented by the three known quantities, s, r, t. Two of these quantities, s and r, may be measured in links or feet on the ground, and the third side may be measured or calculated according to the circumstances of the case. Again, place the target-staff at Z as near the required point K as you are able to judge; but it does not matter where it is placed, as I have before observed, so that it is in the line FK. Then read off the height ZV without changing the position of the instrument at X; from the height ZV take TF, the remainder ZR is one of the sides of the triangle FZR; put this known height ZR $=a$; measure ZF with a tape or chain, and put it $=c$; RF may be measured and put $=e$, or calculated, for $e=\surd(c^2-a^2)$. As on the other side of the centre F, the triangle FRZ is similar to the triangle FLK.

$$\text{Hence FK} : \text{KL} : \text{LF} = c : a : e.$$

To render this method of proceeding as clear as possible, I have dwelt on every point of the process, so that there could be no misunderstanding. This subject has been treated by writers in a most slovenly manner; and by most of the empirical rules laid down by them, it takes three or four trials to determine the position of a side stake.

$$\text{Let KF} = x \text{ and FM} = y.$$

$$\text{AB} = b;\ \text{FC} = h;\ \text{BI} : \text{ID} = m : n.$$

And as we have just found by the level and target-staff that

$$\mathrm{FN} : \mathrm{NM} = t : r,$$

$$\mathrm{FM} : \mathrm{FN} = s : t;$$

Also,

$$\mathrm{FL} : \mathrm{LK} = c : a,$$

$$\mathrm{FK} : \mathrm{KL} = c : a.$$

I have selected these measures in the most general manner, in order that the result may embrace all like cases.

$$c : a : : x : \frac{ax}{c} = \mathrm{KL}.$$

$$\mathrm{KL} : \mathrm{LH} = \mathrm{KJ} : \mathrm{JA} = n : m;$$

$$\therefore \frac{ax}{c} : \mathrm{LH} : : n : m; \text{ or } \frac{amx}{cn} = \mathrm{LH}.$$

$$\mathrm{FH} - \mathrm{HL} = \mathrm{LF}.$$

$$\mathrm{FH} = \tfrac{1}{2}b + \frac{mh}{n};$$

$$\therefore \ \mathrm{LF} = \tfrac{1}{2}b + \frac{m}{n}h - \frac{amx}{cn}$$

$$c : e : : x : \frac{ex}{c} = \mathrm{FL}$$

$$\therefore \frac{ex}{c} = \frac{b}{2} + \frac{mh}{n} - \frac{amx}{cn}$$

$$\therefore \ x = \frac{c(nb+2mh)}{2(en+am)}$$

and hence the exact distance of the side stake K from the centre F becomes known.

$$s : r : : y : \frac{ry}{s} = \mathrm{MN}.$$

$$\mathrm{BI} : \mathrm{ID} = \mathrm{DN} : \mathrm{NM} = m : n,$$

$$\therefore \ n : m : : \frac{ry}{s} : \frac{mry}{ns} = \mathrm{DN}.$$

$$\mathrm{FD} + \mathrm{DN} = \mathrm{FN}.$$

$$\mathrm{FD} = \tfrac{1}{2}b + \frac{mh}{n};$$

$$\therefore \ \mathrm{FN} = \frac{b}{2} + \frac{mh}{n} + \frac{mry}{ns}; \text{ but,}$$

$$s : t : : y : \frac{ty}{s} = \mathrm{FN};$$

$$\therefore \ \frac{ty}{s} = \frac{b}{2} + \frac{mh}{n} + \frac{mry}{ns}$$

$$y = \frac{s(nb+2mh)}{2(nt-mr)};$$

and hence the exact distance of the side stake M is readily determined.

Example.—Given the breadth of the roadway AB = 28 feet; the height of the centre stake CF $= h = 24$ feet; the ratio of the slopes AH, BM, or BI : ID = 3 : 2. In a surface distance FY = 60 feet $= s$. A rise FQ = 7 feet $= r$, is found by the level and target staff; in the surface distance, FZ = 50 feet $= c$, a fall RZ = 4 feet $= a$ is found. Required the points KM where the surface of the ground intersects the slopes that form the road. The solution of this problem and of this first example are given at full length, in order that the ground-work of the practical rule, to be given presently, may be well understood.

$$\text{FM} : \text{FN} : \text{MN} = s : \sqrt{(s^2 - r^2)} : r,$$

$$\text{FK} : \text{FL} : \text{LK} = c : \sqrt{(c^2 - a^2)}\,;\ a.$$

$$60^2 = 3600$$

$$7^2 = 49$$

$$t = \sqrt{s^2 - r^2} = \sqrt{3551} = 59{\cdot}59$$

$$50^2 = 2500$$

$$4^2 = 16$$

$$e = \sqrt{c^2 - a^2} = \sqrt{2484} = 49{\cdot}84$$

$$x = \frac{50(2 \times 28 + 2 \times 3 \times 24)}{2(49{\cdot}84 \times 2 + 4 \times 3)} = \frac{10000}{223{\cdot}36} = 44{\cdot}8 \text{ feet.}$$

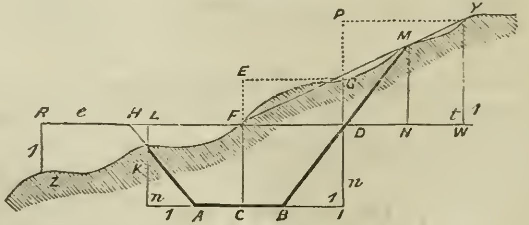
= the distance from F to K.

$$y = \frac{60(2 \times 28 + 2 \times 3 \times 24)}{2(59{\cdot}59 \times 2 - 7 \times 3)} = \frac{12000}{196{\cdot}36} = 61{\cdot}11$$

the distance from F to M. Although this calculation is simple and extremely accurate, yet the generality of practical men require rules that can be applied without entering into the reasoning of the matter in every particular case and example. To suit this class of practitioners I will lay down one or two other methods of finding the values of x and y.

In Fig. 6, by the use of the level and target-staff, as described in Fig. 5, let the rise from the centre stake F, in the direction FY, be determined; which represent by the ratio $t : 1$. That is, FW : WY : $t : 1$.

Fig. 6.



In the same manner, by placing the target-staff at Z, in the neighborhood of K, find the fall from the centre stake F, so that F,Z,K, may be in the same straight line, or nearly equal so; in general terms this ratio may be represented by e to 1, that is,

$$\text{FR} : \text{RZ} : : e : 1.$$

Before going on the ground to set out side stakes, the lines of the figure HABD are known; the horizontal half breadths FD, FH, are found from the height FC, and the breadth of the roadway AB, being given. When the positions of H and D are known, it is not difficult to select some points, Z and Y, near them to ascertain the slope of the ground. In finding the half breadth FD and FH, the ratio of BI to ID is also given; this ratio may be represented by $1 : n$, that is

$$\text{BI} : \text{ID} = 1 : n;\ \text{DN} : \text{NM} = 1 : n;$$
$$\text{HL} : \text{LK} = 1 : n;$$

Put $v = \text{HL}$, then $1 : n : : v : nv = \text{LK}$.

Put $\text{HF} = d = \text{FD}$, the half horizontal breadth, through the centre stake F.

$$e : 1 : : d - v : \frac{d-v}{e} = \text{KL};\quad \therefore\ nv = \frac{d-v}{e} \text{ and } v = \frac{d}{ne+1}.$$

Again, put $\text{DN} = z$, then

$$1 : n : : z : nz = \text{MN};$$
$$\text{and } t : 1 : : d + z : \frac{d+z}{t} = \text{MN}.$$
$$\therefore\ nz = \frac{d+z}{t}, \text{ and } z = \frac{d}{nt-1}.$$

From which the following simple practical rule may be deduced.

RULE.—When the ground rises from the centre, increase the horizontal half breadth, divided by the half breadth by the product, (less one) of the numbers that express the ratio of the rise and the ratio of the slope, and the horizontal distance of the side stake is determined. When the ground falls from the centre, decrease the horizontal half breadth, by the half breadth divided by the product (plus one) of the numbers that express the ratio of the rise and the ratio of the slope; and the horizontal distance of the other side stake is found.

Example—Given the breadth of a railway $\text{AB} = 30$ feet; the height $\text{CF} = 26$ feet; the side slopes $1 : 2$ ($\text{BI} : \text{ID} : : 1 : 2$); the rise of the surface from F to $\text{Y} = 1$ in 20 ($\text{FW} : \text{WY} : : 20 : 1$). The fall from F to $\text{D} =$ 1 in 36; required the horizontal distances FN and FL, where the surface of the ground intersects the side slopes of the railroad.

$\text{HF} = \frac{30}{2} + \frac{1}{2}\, 26 = 28 = \text{FD}$, half the horizontal breadth meeting the side slopes on a level line through F. The horizontal distance FW may be measured in lengths EG, PY, when the surface is irregular.

$$2 \times 20 - 1\ 39, \text{ and } \tfrac{28}{39} = \cdot 72$$
$$\therefore\ \text{FN} = 28 \cdot 72 \text{ feet}$$
$$2 \times 36 + 1 = 73, \text{ and } \tfrac{28}{73} = \cdot 38,$$
$$\therefore\ 28 - \cdot 38 = 27 \cdot 62 = \text{FL}.$$

This calculation is so plain and simple, that the positions of the side stakes are found in a few seconds. It should be noticed that in the ratios of the slopes, and inclination of the ground, unity is taken for the base of the side slopes, but for the perpendicular of the rise or fall of the ground.

(To be Continued.)

For the Journal of the Franklin Institute.

Suspension and Arch Truss Bridges, according to a New System.

By S. W. Robinson, C. E., Detroit, Mich.

In the previous article upon "*Suspension Bridges*," published in your last volume—which was prepared by De Volson Wood, Professor of Civil Engineering University of Michigan, from my thesis left there at the last college commencement—the tangent line to the arc at the lowest point was taken as the line of attachment of the normal suspending ties to the uniform horizontal loading. In that case the tension was found to be exactly equal to the tension at the pier heads of the ordinary form with parabolic arc.

I have since solved the problem in a more general way. I have supposed the cable to pass above, or below, the line of attachment to the loading.

A diminished tension is the result of passing the cable entirely or in part below the roadway; the span and versed sine remaining the same.

This inverted is the form for an arch truss, with a uniform and diminished stress throughout the arched chords.

Now let A E, Fig. 1, represent the arc of the cable, passing at a distance a, below the line C D of attachment to the loading.

Let A be the origin of co-ordinates. AF and HG equal elements of arc.

w = the weight of loading per unit of length.

wM^1 the amount to which pressure is due upon AF, and

wM the amount to which pressure is due upon HG.

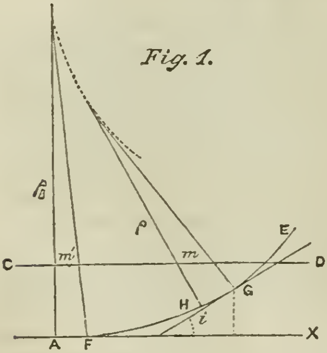


Fig. 1.

Then

$$M^1 = \frac{AF(\rho_0 - a)}{\rho_0}. \qquad . \qquad . \qquad . \; (A)$$

and

$$M = \frac{HG(\rho - (a - y)\sec i)}{\rho \cos i}.$$

According to theorem II of previous article

$$wM^1 \rho_0 = wM \rho \sec i, \qquad - \qquad . \qquad (1)$$

or

$$AF(\rho_0 - a) = \rho M \sec i = HG(\rho - (a - y)\sec i)\sec^2 i.$$

But AF was assumed equal HG.

$$\therefore \quad (\rho_\circ - a) = (\rho - (a-y)\sec i)\ \sec^2 i.$$

Put $u = \frac{dy}{dx}$, then $\frac{du}{dx} = \frac{d^2y}{dx^2} = \frac{udu}{dy}$ and $\rho = \frac{(1+u^2)^{\frac{3}{2}}}{\frac{udu}{dy}}$, $\sec i = \sqrt{1+u^2}.$

Hence (2) becomes by substituting and reducing,

$$(1+u^2)^{\frac{5}{2}} - (a-y)\ (1+u^2)^{\frac{3}{2}}\ \frac{udu}{dy} = (\rho_\circ - a)\frac{udu}{dy}, \qquad . \qquad (3)$$

or,

$$dy + y\ \frac{udu}{1+u^2} = \left\{ \frac{a}{1+u^2} + \frac{\rho_\circ - a}{(1+u\ \ )^{\frac{5}{2}}} \right\} udu.$$

The integral of this, treated as a linear equation after correcting by determining the constant, and reducing, is,

$$y = a + \frac{\rho_\circ - 3a}{2(1+u^2)^{\frac{1}{2}}} - \frac{\rho_\circ - a}{2(\rho_\circ + u^2\ )^{\frac{3}{2}}}, \qquad . \qquad . \qquad (4)$$

or

$$(1+u^2)^{\frac{3}{2}} = \frac{1}{2(a-y)} \left\{ 2a - u^2\ (\rho_\circ - 3a) \right\}.$$

This in the second term of (3) makes it

$$dx - \frac{adu}{(1+u^2)^{\frac{5}{2}}} + \frac{u^2(\rho_\circ - 3a)du}{2(1+u^2)^{\frac{5}{2}}} = \frac{(\rho_\circ - a)du}{(1+u^2\ )^{\frac{5}{2}}}.$$

The integral of this is

$$x = \frac{u\rho_\circ}{(1+u^2\ )^{\frac{1}{2}}} - \frac{u^3\ (\rho_\circ - a)}{2\ (1+u^2\ )^{\frac{3}{2}}}, \qquad . \qquad (5)$$

I have not attempted the complete integration of (3) as a function of x, and y, only. Much more simple expressions for the ordinates and abscissas, y and x, are obtained by introducing the variable i.

Hence, since $u = \frac{dy}{dx} =$ tang i, (4) becomes

$$y = a - \cos i \left\{ a - \tfrac{1}{2}(\rho_\circ - a)\sin^2 i \right\}. \qquad . \qquad (6)$$

And (5) becomes

$$x = \sin i \left\{ \rho_\circ - \tfrac{1}{2}(\rho_\circ - a)\sin^2 i \right\}. \qquad . \qquad . \qquad (7)$$

Brief discussion of the Curve.

Differentiating (6) or (7) and either gives for maximum ordinates,

$$\sin i = \pm \sqrt{\frac{2\rho_\circ}{3(\rho_\circ - a)}}, \qquad . \qquad . \qquad (8)$$

two equal values with contrary signs.

The tests for a cusp will be satisfied for both these values.

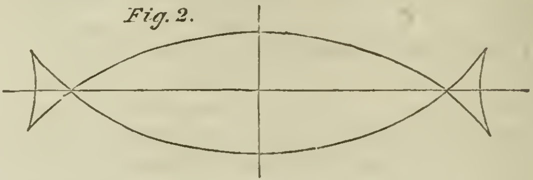
Make $i=90°$. Then $\sin i=1$ $\cos i=0$. Hence,

$$y=a,\ x=\tfrac{1}{2}(\rho_0+a.)$$

For $i=180°$, $y=2a$, $x=0$.

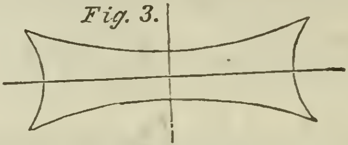
Hence, the curve is symmetrical, with respect to an axis parallel to X and at a distance a, from it. Where a is positive, the curve is as represented in Fig. 2.

Fig. 2.



When a is negative, the curve is as in Fig. 3.

Fig. 3.



Length of Arc.

$$ds=\frac{dy}{\sin i}=-\tfrac{1}{2}(\rho_0-3a)di+\tfrac{3}{2}(\rho_0-a)\cos^2 i di$$

$$\therefore\ s=\tfrac{1}{4}\left\{(\rho_0+3a)i+\tfrac{3}{2}(\rho_0-a)\sin 2\,i\right\}. \quad . \quad (9)$$

Length of Normal Ties.

Let $l=$ the length. Then,

$$l=(a-y)\sec i=a-\tfrac{1}{2}(\rho_0-a)\sin^2 i. \quad . \quad (10)$$

The point of attachment of the Ties to the roadway.

Let b=the distance from the centre to any tie. Then,

$$b=x-(a-y)\ \text{tang}\ i=(\rho_0-a)\sin i. \quad . \quad . \quad . \quad (11)$$

Stress of the Ties.

Let P be the total stress from the origin to any point considered. Then,

$$P=w\int db\sec i,=w(\rho_0-a)i \quad . \quad . \quad (12)$$

by substituting db, from (11) and integrating.

Horizontal stress along the roadway caused by the inclined ties.

Let H=the required stress,

$$\therefore\ H=w\int db\ \text{tang}\ i=w\int\frac{bdb}{\sqrt{(\rho_0-a)^2-b^2}}=w(\rho_0-a-\sqrt{(\rho_0-a)^2-b^2} \quad (13)$$

Tension of the Cable.

In equation (A) let AF=unity.

Then by theorem II, and equation (2) following, of my previous article

$$\text{T}=\text{WM}^1\,\rho_\circ,\ \text{or} =\text{W}(\rho_\circ-a). \quad . \quad . \quad (14)$$

If in any of the preceding formulæ we make $a=0$, *we get expressions identical to those of the article referred to in the last volume.*

In that article the same expression was obtained for the tension of cable as in the ordinary suspension bridge at the pier heads. But when it passes below the roadway the tension is sensibly diminished.

Take the case where the line of attachment passes through the pier heads. Then this line is equal to the span. Put it=2 B.

In (10) make $l=0$.

$$\therefore\ \sin i=\sqrt{\frac{2a}{\rho_\circ-a}}, \quad . \quad . \quad . \quad (15)$$

This in (11) gives

$$\text{B}\dot{=}\sqrt{2a(\rho_\circ-a)}.$$

$$\therefore\ \rho^\circ=\frac{\text{B}^2}{2a}+a. \quad . \quad . \quad . \quad . \quad (16)$$

This in (14) gives

$$\text{T}=\frac{\text{WB}^2}{2a}. \quad . \quad . \quad . \quad . \quad (17)$$

That is, the tension in this case is exactly equal to the tension in the common parabolic system at the lowest point of the arc. Hence, there is a diminution of

$$\frac{\text{WB}}{2a}\left(\text{B}^2+4a^2\right)^{\frac{1}{2}}-\frac{\text{WB}^2}{2a}$$

or

$$\text{T}^1-\text{T}=\frac{\text{WB}}{2a}\left\{\left(\text{B}^2+4a^2\right)^{\frac{1}{2}}-\text{B}\right\}$$

at the pier heads.

But I believe the foregoing principles to be of more practical utility in application to *Truss Girders* with one-arched chord than to suspension bridges.

An arched chord made in parabolic arc with vertical ties and uniform loading, must increase in size from the crown to the abutments according to the formula

$$\text{T}^1=\frac{\text{WB}}{2a}\left(\text{B}^2+4y^2\right)^{\frac{1}{2}}$$

At the apex $y=o$, and

$$\text{T}^1=\frac{\text{WB}^2}{2a}.$$

But in the system with the normal ties the stress is uniform throughout and $=\frac{\text{WB}^2}{2a}$, *the same as in the parabolic arc at the apex.*

In view of this, I have made investigation and derived formulæ necessary in the construction of arch truss girders according to the new system, which are given below.

The arched chord should not consist of portions of arc; but of the chords of those arcs between the ties and braces. And that the stress of each may be constant, the angle of any two adjacent parts should be constant, that the principle of the parallelogram of forces may be satisfied. Therefore the strain upon the ties must be the same for a uniform load throughout. Hence let it be required to find the co-ordinates of the points at which to join the ties with the arched and straight chords, that the above conditions may be fulfilled.

Equation (11) gives $\sin i = \frac{\text{B}}{\rho_0 - a} = \frac{2a}{\text{B}}$:

$$\therefore \quad i = \sin.^{-1}\frac{2a}{\text{B}}, \qquad \qquad (18)$$

for the point of intersection of the chords.

And since the angles of intersection of the broken chord must be equal, we have for the angle of each,

$$\frac{2i}{2\text{N}+1} = i_1 \text{ and } \frac{2ni}{2\text{N}+1} = i_n = n\,i_1$$

In which $2\text{N}=$ the total number of ties.

This value of i_n and (16) in (6) and (7) give for the points in the arched chord,

$$y_n = a - \cos.\, i_n \left\{ a - \frac{\text{B}^2}{4a} \sin.^2 i_n \right\} \qquad . \qquad . \qquad (19)$$

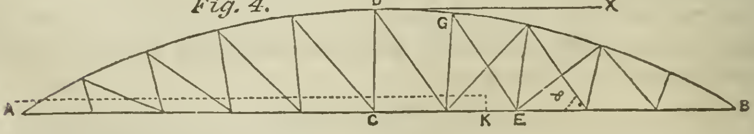
$$x_n = \sin.\, i_n \left\{ \frac{\text{B}^2}{2a} + a - \frac{\text{B}^2}{4a} \sin.^2 i \right\} \qquad . \qquad . \qquad (20)$$

From (11) for the horizontal chord we have

$$b_n = \frac{\text{B}^2}{2a} \sin. i_n .$$

Dimensions of the parts.

Fig. 4.



Let $\text{L}=$length of a brace

$b'_{\text{n}}=$the distance from centre to middle of bay considered,

$\text{C}=$length of one part of the broken chord.

$$i'_1 = \tfrac{1}{2} i_1 \;\therefore\; i'_{\text{n}} = (n - \tfrac{1}{2}) i_1$$

Other notation as before.

The length of a tie (10) is

$$l_n = a - \tfrac{1}{2}(\rho_0 - a)\sin^2 i_n$$

$$= a - \frac{B^2}{4a}\sin^2 i_n \qquad . \qquad . \qquad . \qquad (21)$$

The length of any portion of the arched chord is

$$C_n = (x_n - x_{n-1})\frac{1}{\cos i'_n} \qquad . \qquad . \qquad (22)$$

The length of a brace is

$$L_n = \left(a - y_{n-1}\right)\frac{1}{\sin \theta_n} \qquad . \qquad . \qquad (23)$$

for which $\tan \theta_n = \dfrac{a - y_{n-1}}{b_n - x_{n-1}}$

Stress of the Parts.

The maximum strain upon the middle of the chord is when the bridge is loaded throughout and is given by (17). This may also be shown geometrically.

This decreases toward the extremities of the horizontal chord according to (13). Hence the strain in the n^{th} bay is

$$\frac{WB^2}{2a} - W\left(\rho_0 - a - \sqrt{(\rho_0 - a)^2 - b_n^2}\right) = \frac{W}{2a}\sqrt{B^4 - 4a^2 b_n^2}. \quad (24)$$

This shows that when $a = \frac{1}{2}B$ the stress of the lower chord is $= 0$ at the abutments for a uniform load. But when the load enters the bridge there is a horizontal thrust given by (26), which *may* exceed that of (24).

The maximum stress of a brace is when the load extends from the middle of the bay considered to the end toward which the brace inclines.

From the middle of bridge to middle of bay considered:

$$b'_n = \frac{B^2}{2a}\sin i'_n \,.$$

Let AK, Fig. 4, represent the load which causes the maximum strain upon the brace EG. Then taking moments about A, the vertical force at B is

$$V_n = \frac{W(B + b'_n)^2}{4B}. \qquad . \qquad . \qquad (25)$$

which is communicated to G by the transverse shearing. The sign of b' changes for $n = 0$, or negative.

Similarly the horizontal thrust at G is

$$H_n = \frac{B - x_{n-1}}{a - y_{n-1}} V_n \,. \qquad . \qquad . \qquad . \qquad (26)$$

in which x becomes positive, when n is negative. The resultant of these two forces, H_n and V_n, is directed at B, which can be shown geometrically, since

$$\frac{V_n}{H_n} = \frac{a - y_{n-1}}{B - x_{n-1}} = \tan \text{EBG}.$$

Hence the stress of the brace is the resultant of these two forces resolved into it.

Let v=the stress of the brace, and h= that of the chord.

$$\therefore \left.\begin{array}{l} v \sin \theta_n + h \sin i' = V \\ v \cos \theta_n + h \cos i' = H \end{array}\right\}$$

From which

$$v_n = \frac{V_n - H_n \operatorname{tang} i'_n}{\sin \theta_n - \cos \theta_n \operatorname{tang} i'_n}$$

in which the negative signs become positive when n is negative.

The maximum stress of the ties *may* be produced by the pressure of the braces.

The total pressure upon half the ties for uniform load (12), is

$$P = \frac{wB^2 i}{2a} = \frac{wB^2}{2a} \sin^{-1} \frac{2a}{B}.$$

Hence the resulting strain upon one tie is $\frac{2P}{2N+1} = p$.

But the stress caused by the braces for partial load is

$$t_n = \frac{V_{n+1} - H_{n+1} \operatorname{tang} i'_n}{\cos i_n + \sin i_n \operatorname{tang} i'_n}$$ in which H is negative when n is negative.

$\therefore$ for $n=0$ $t = \frac{wB}{4}$ nearly, which is greater than p unless 2N be less than 8 or 9, in ordinary cases.*

If it should be desired to substitute diagonal ties for the braces, expressions for the stress of the various part scould be derived similarly as the above.

It might be added that a can never exceed $\frac{1}{2}$B, for in this case the position for the cusp would be below the roadway in the suspension, and above in the arch truss bridge. Equation (8) would also become imaginary.

In the case when $a = \frac{1}{2}$B there is no cusp for the points, for each is coincident, and in the roadway. Hence for this case the curve is continuous, and intersects its axis but once on each side of the origin.

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* If $B=8$, $a=2$, $w=1$, $7=2N$, then $i=30°$, $2P=16{\cdot}755+$ $\frac{2P}{2N+1} = 2{\cdot}09+$, and $t = 1{\cdot}868+$, which is less than p: but if $2N = 10$, t will be greater than p.

For the Journal of the Franklin Institute.

Omissions in a Closed Survey. By De Volson Wood, Prof. of C. E. University of Michigan.

It is well known that, if certain omissions are made in a *Closed Survey*, they may be supplied from the remaining notes. I propose in this article to investigate the subject analytically.

I wish at the outset to express my acknowledgments to Mr. Cleveland Abbe, formerly a tutor in the University, and now connected with the United States Coast Survey, for the free use which he has permitted me to make of his notes upon this subject. His analysis covered nearly the whole ground, so that I have only to make slight additions and changes, and add geometrical illustrations, to put it in the form in which it is now presented.

In order to convert the bearings taken in the field into angles, which will be more convenient for our computations, we have

The bearing N β°E is equivalent to the angle β°
" " S β°E " " " $180^\circ-\beta^\circ$
" " S β°W " " " $180^\circ+\beta^\circ$
" " N β°W " " " $360^\circ-\beta^\circ$

and conversely an angle β being given we have when

$\beta^\circ < 90^\circ$ and $> 0^\circ$ the bearing is N β°E.
$\beta^\circ > 90^\circ$ and $< 180^\circ$ " " S $180^\circ-\beta^\circ$E.
$\beta^\circ < 270^\circ$ and $> 180^\circ$ " " S $\beta^\circ-180^\circ$W.
$\beta^\circ > 270^\circ$ and $< 360^\circ$ " " N $360^\circ-\beta^\circ$W.

Let l_1; l_2; l_3; &c., be the lengths of the known sides.
x_1; x_2 " " unknown sides.
b_1; b_2; b_3; &c., the known angles as found from the known bearings as given above.
β_1; β_2; . . the unknown angles.
D = the algebraic sum of all the known departures.
L = the algebraic sum of all the known latitudes.

Then for a closed field we evidently have

$$\left.\begin{array}{l}\Sigma\ \text{Dep.} = l_1 \sin b_1 + l_2 \sin b_2 + \ \ldots\ l_n \sin b_n = 0 \\ \Sigma\ \text{Lat.} = l_1 \cos b_1 + l_2 \cos b_2 + \ \ldots\ l_n \cos b_n = 0\end{array}\right\} \quad . \qquad (1)$$

From which any two quantities may generally be found when all the others are known. We have several cases.

Case I.

Let One Bearing be Omitted.

Letting l=the length of any side, and we have from (1)

$$\left.\begin{array}{l} l \sin.\ \beta_1 = \text{D} \\ l \cos.\ \beta = \text{L}_1 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

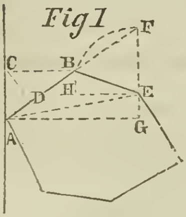
$$\therefore \sin.\ \beta_1 = \frac{\text{D}}{l} \quad \cos.\ \beta_1 = \frac{\text{L}}{l}. \qquad . \quad . \quad . \quad . \qquad (3)$$

and β_1 obtained from each of those must be the same. We also have tang. $\beta_1 = \frac{\text{D}}{\text{L}}$, which should give the same value as each of the others.

Eliminating l from (2) gives

$$\text{D} \cos.\ \beta - \text{L} \sin.\ \beta = 0 \qquad . \quad . \quad . \quad . \qquad (4)$$

These may be deduced geometrically; thus, let the bearing of AB, Fig. 1, be unknown.



Then CB=D. AC=L, and,

$$\sin.\ \text{CAB} = \sin.\ \beta_1 = \frac{\text{CB}}{\text{AB}} = \frac{\text{D}}{l}$$

$$\cos.\ \text{CAB} = \cos.\ \beta_1 = \frac{\text{AC}}{\text{AB}} = \frac{\text{L}}{l}$$

$$\tan.\ \text{CAB} = \tan.\ \beta_1 = \frac{\text{CB}}{\text{AC}} = \frac{\text{D}}{\text{L}}$$

From C let fall CD, perpendicular to AB. Then

$$\text{CD} = \text{CB} \sin \text{CBD} = \text{CB} \cos.\ \text{CAD} = \text{D} \cos.\ \beta_1$$

Also $\text{CD} = \text{CA} \sin \text{CAD} = \text{L} \sin.\ \beta_1$

By subtracting these we have $\text{D} \cos.\ \beta_1 - \text{L} \sin.\ \beta_1 = 0$
All these are the same as the analytical results.

Case II.

Let One Distance be Omitted.

Then Eq. (1) gives, if b be any angle,

$$x_1 \sin.\ b = \text{D} \qquad . \quad . \quad . \quad . \qquad (5)$$

$$x_1 \cos.\ b = \text{L} \qquad . \quad . \quad . \quad . \qquad (6)$$

$$\therefore x_1 = \frac{\text{D}}{\sin.\ b} = \frac{\text{L}}{\cos.\ b}$$

Squaring (5) and (6) and adding, gives

$$x^2 = \text{D}^2 + \text{L}^2$$

In Fig. 1, $\text{AB} = \text{CA} \div \cos \text{CAB}$ or $x_1 = \frac{\text{L}}{\cos.\ b}$

also $\text{AB} = \text{CB} \div \sin \text{CAB}$ or $x_1 = \frac{\text{D}}{\sin.\ b}$

also $\text{AB}^2 = \text{AC}^2 + \text{BC}^2$,
or $x_1{}^2 = \text{L}^2 + \text{D}^2$.

Case III.

The Length and Bearing of One Side Omitted.

From (1) we have

$$\left.\begin{aligned} x_1 \sin \beta_1 &= \text{D} \\ x_1 \cos \beta_1 &= \text{L} \end{aligned}\right\} \qquad . \quad . \quad . \qquad (7)$$

From these we readily find

$$\tan \beta_1 = \frac{\text{D}}{\text{L}}$$

$$x_1 = \sqrt{\text{D}^2 + \text{L}^2}$$

These results have already been illustrated geometrically.

Case IV.

The Length of One Side and Bearing of Another Wanting.

For this Eq. (1) gives

$$x_1 \sin b_1 + l_2 \sin \beta_2 = \text{D} \qquad . \qquad . \qquad (8)$$

$$x_1 \cos b_1 + l_2 \cos \beta_2 = \text{L} \qquad . \qquad . \qquad (9)$$

Multiply Eq. (8) by $\cos b_1$ and (9) by $\sin b_1$ and subtract the results, and we find

$$l_2 \sin (\beta_2 - b_1) = \text{D} \cos b_1 - \text{L} \sin b_1 \qquad . \qquad . \qquad (10)$$

from which we may find two values for $\beta_2 - b_1$, viz: $\theta°$ and $180° - \theta°$.

$$\therefore \quad \beta_2 = \theta + b_1 \text{ and } 180° - \theta° + b_1$$

From (8) and (9) we find

$$x_1 = \frac{\text{D}}{\sin b_1} - \frac{l_2}{\sin b_1} \sin \beta_2 \qquad . \qquad . \qquad . \qquad (11)$$

$$x_1 = \frac{\text{L}}{\cos. b_1} - \frac{l_2}{\cos. b_1} \cos. \beta_2$$

in which if both values of β_2 be substituted, we will obtain two values of x_1.

From (10) we find that β_2 (and therefore x_1) is imaginary when

$$\frac{\text{D} \cos. b_1 - \text{L} \sin. b_1}{l_2} > 1.$$

If $\beta_2 - b_1 = 90°$ there is only one solution.

If the second member of (10) is zero, we have $\beta_2 = b_1$; or the two courses coincide in direction, and we have from (8) or (9)

$$x_1 + l_2 = \frac{\text{D}}{\sin. b_1}$$

and the case is essentially the same as II.

To show this geometrically, we first observe that the two courses may be adjacent or separated. If separated, we may transfer the sides so as to bring the desired ones adjacent.

In fig. 1, let the length of AB and bearing of BE be wanting. Then by Case III compute the bearing and length of a line which joins A and E. Lay off the line AB according to the given bearing. Then with E as a centre and radius equal BE, describe an arc which will intersect AB in two points, B and F; hence there will be two lengths, AB and AF; and two bearings, one for BE, the other for FE. If the arc BF is tangent to AB, there will be but one solution; and if the arc does not touch AB, the solution is impossible, and the survey is not closed.

In the figure draw HE and AG perpendicular, and BH and EG parallel to AC. Then

$$\text{AG} = \text{CB} + \text{HE},$$

or

$$\text{D} = x_1 \sin. b - l_2 \sin. \beta_2.$$

The sign of sin. β_2 is minus because the angle is obtuse. From this equation we find

$$x_1 = \frac{\text{D}}{\sin. b_1} - \frac{l_2}{\sin. b_1} \sin. \beta_2 \text{ as before.}$$

CASE V.

Let the Lengths of Two Sides be Wanting.

Equation (1) gives

$$x_1 \sin. b_1 + x_2 \sin. b_2 = \text{D} \quad . \quad . \quad . \quad . \quad (12)$$

$$x_1 \cos. b_1 + x_2 \cos. b_2 = \text{L} \quad . \quad . \quad . \quad . \quad (13)$$

Multiply (12) by cos. b_2 and (13) by sin. b_2 and take the difference, and we obtain

$$x_1 = \frac{\text{D} \cos. b_2 - \text{L} \sin. b_2}{\sin. (b_2 - b_1)} \quad . \quad . \quad (14)$$

Similarly,

$$x_2 = \frac{\text{D} \cos. b_1 - \text{L} \sin. b_1}{\sin. (b_2 - b_1)} \quad . \quad . \quad (15)$$

If these results are negative, the lines are imaginary and the solution is impossible—in other words, it is not a closed survey.

If $b_2 = 180 + b_1$, the unknown lines are parallel, and the denominator becomes zero; but the numerator also becomes zero at the same time—see (Eq. 4)—hence $x_2 = \frac{0}{0}$, which is undeterminate.

When a solution is possible there is no ambiguity.

In fig. 2, let the lengths of AB and BC be unknown. By Case III compute AC. Then draw AB and BC with the known bearings, and their point of intersection will give the point B, and AB will be the required length of one line, and BC of the other. If the bearing of BC should cause it to run in the opposite direction, it would not meet AB, and the solution would be impossible.

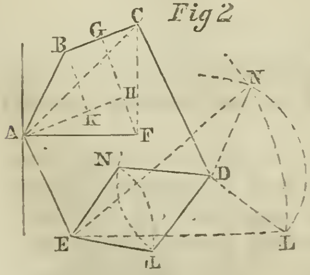


Fig 2

Draw AF perpendicular, and CF parallel to AM; AH parallel to BC, and FB perpendicular to BC. Then

$$\text{BAH} = b_2 - b_1.$$

$$\text{BK} = \text{GH} = x_1 \sin. (b_2 - b_1).$$

$$\text{FG} = \text{CF} \sin. \text{BCF} = \text{L} \sin. b_2.$$

$$\text{FH} = \text{AF} \cos. \text{AFH} = \text{D} \cos. b_2.$$

$$\therefore \text{GH} = \text{FG} - \text{FH} =$$

$$\text{L} \sin. b_2 - \text{D} \cos. b_2 = x_1 \sin. (b_2 - b_1).$$

$$\therefore x_1 = \frac{\text{D} \cos. b_2 - \text{L} \sin. b_2}{- \sin. (b_2 - b_1)} \text{ as before.}$$

If AE and CD are the unknown lengths, and are parallel, it is evident that the bearings and lengths of all the other lines may be the same for all lengths of these lines; provided only that they have a constant difference, hence they are indeterminate.

Case VI.

Let the Bearings of Two Sides be Wanting.

Then
$$l_1 \sin \beta_1 + l_2 \sin \beta_2 = \text{D} \qquad . \qquad . \qquad . \tag{16}$$
$$l_1 \cos \beta_1 + l_2 \cos \beta_2 = \text{L} \qquad . \qquad . \qquad . \tag{17}$$

These give
$$l_1 \sin \beta_1 = \text{D} - l_2 \sin \beta_2.$$
$$l_1 \cos \beta_1 = \text{L} - l_2 \cos \beta_2.$$

Squaring these and adding gives
$$l_1^2 = \text{D}^2 + \text{L}^2 + l_2^2 - 2\, l_2 (\text{D} \sin \beta_2 + \text{L} \cos \beta_2).$$

Similarly
$$l_2^2 = \text{D}^2 + \text{L}^2 + l_1^2 - 2\, l_1 (\text{D} \sin \beta_1 + \text{L} \cos \beta_1).$$

In the first of these we might substitute $\cos \beta_2 = \sqrt{1 - \sin^2 \beta_2}$ and reduce, but the expression would be very lengthy. Instead of that we will make

$$\text{D} = m \cos \theta \qquad . \qquad . \qquad . \qquad . \tag{18}$$
$$\text{L} = m \sin \theta \qquad . \qquad . \qquad . \qquad . \tag{19}$$

and we shall find

$$\sin (\theta + \beta_2) = \frac{m^2 + l_2^2 - l_1^2}{2\, m\, l_1} \qquad . \qquad . \tag{20}$$

$$\sin (\theta + \beta_1) = \frac{m^2 - (l_2^2 - l_1^2)}{2\, m\, l_1} \qquad . \qquad . \tag{21}$$

From which we find two values for $(\theta + \beta_2)$ and two for $(\theta + \beta_1)$; hence there are two solutions. If $\theta + \beta_2 = 90°$ or $180°$, there is but one solution, and the two bearings will equal each other.

In fig. 2, let EL and DL be the sides whose bearings are unknown. With E and D as centres and radii equal the lengths of the sides describe arcs, and the points where they intersect will be the corners of the field. As they will generally intersect in two points, there will generally be two solutions. EN or EL will be one side; and DN or DL the other. If the arcs are tangent to each other there will be but one solution, and the two sides will coincide in direction.

As an illustration of the manner of using these equations, take the following examples, found in Gillespie's Surveying. In Article 441 of his Surveying we have given

$$\text{D} = -\,5314{\cdot}34587 \qquad l_2 = +\,4621{\cdot}5$$
$$\text{L} = -\,1405{\cdot}32477 \qquad b_1 = \quad 198°$$

This problem belongs to Case IV.

Now apply Eq. (10).

$$\begin{array}{rlcr}
 & \text{D} \cos b_1 & = & +\,5054{\cdot}244 \\
 & \text{L} \sin b_1 & = & +\ \ 434{\cdot}269 \\
\cline{4-4}
 & \log \text{ of dif.} & = & 3{\cdot}664641 \\
 & \log b_2 & = & 3{\cdot}664783 \\
\cline{4-4}
\therefore & \log \sin (\beta_2 - b_1) & = & 9\ 999858 \\
\therefore & \beta_2 - b_1 & = & 88°\ 32' \text{ or } \ 91°\ 28' \\
\therefore & \beta_2 & = & 286°\ 32' \text{ or } 289°\ 28'
\end{array}$$

Corresponding bearing is N. 73° 28′ W., or N. 70° 32′ W.
Now use Eq. (11).

Log sin β_2	=	n 9·981662	or n 9·974436
Log l_2	=	3·664783	3·664783
Sum of log	=	n 3·646445	n 3·639219
Log sin b_1	=	n 9·489982	n 9·489982
Log D	=	n 3·725450	n 3·725450
D cosec b_1	=	+ 17197·6	+ 17197·6
l_2 sin β_2 cosec b_1	=	14337·16	14100·58
∴ x_1	=	+ 2860·44	or + 3097·02

For an example under Case VI, take the one in Article 448 of Gillespie. We have

$$\text{D} = +1479{\cdot}75010 \qquad l_1 = +2400$$
$$\text{L} = -2303{\cdot}26591 \qquad l_2 = +2860$$

Apply Eqs. (18), (19), (20), and (21).

Log. m sin. θ n 3·362344 — l_2^2 + 8179600
log. sin. θ n 9·924967 — log. m 3·437377 — l_1^2 + 5760000
log. m cos. θ 3·170189 — log. m^2 6·874754

+ 2419600
θ = 302° 43′ 8·7″ — m^2 + 7494700

log. $(m_1^2 + l_2^2 - l_1^2)$	6·996262	log. $(m^2 - l_2^2 + l_1^2)$	6·705445
av. comp. 2 m_2	6·261613		6·261613
av. comp. l_2	6 543634	av. comp. l_1	6·619789
sin. $(\theta + \beta_2)$	9·801509	sin. $(\theta + \beta_1)$	9·586847
∴ $\theta + \beta_2$ =	39° 16′ 59·2″ or 140° 43′ 0·8″	∴ $\theta + \beta$ =	22° 43′ 22·6″ or 157° 16′ 37·4″
θ =	302 43 8·7 or 302 43 8·7		302 43 8·7 or 302 43 8·7
β_2 =	96 33 50·5 or 197 59 52·1	β_1 =	80 0 13·9 or 214 33 28·7

Bear'g = S 83° 26′ 9·5″ E or S 17° 59′ 52·1″ W. Br'g. = N 80° 0′ 13·9″ E or S 34° 33′ 28·7″ W

MECHANICS, PHYSICS, AND CHEMISTRY.

Steam Engine Economy.

From the Lond. Mechanics' Magazine, Jan., 1864.

A patent case has recently been tried at Washington, U. S., which has elicited certain statements so remarkable in their bearing on the system of steam engine construction adopted of late in the American Navy, that we cannot pass it without notice. The case in question is simple enough in itself. A Mr. Mattingly, of Washington, sued a steamboat company for a share of the savings effected by a peculiar form of cut-off valve, the patent right in which he held, and had sold to the company, taking a share of the savings effected as the pecuniary consideration. The defence set up was singular enough. The complainant asserted, and as he believed proved, that the saving in fuel amounted to over 30 per cent., and he demanded a verdict in accordance with his statement. The defendants, however, called Mr. Benjamin Isherwood, the Chief Engineer of the United States Navy, to prove not only that the particular cut-off which afforded the *casus belli* could not possibly effect the alleged saving; but that the only possible saving which could be effected by that, or any other cut-off, amounted at the maximum to 18 per cent., and that, therefore, a saving of thirty per cent. was a physical impossibility. This statement is startling enough; but Chief Engineer Isherwood does his work thoroughly; and he proceeded to strengthen his evidence by swearing that no one believed his "new discovery" to be true, until 1860, and that since then all the new marine engines for the navy were built and building upon it. It is nothing new to us to learn that American engines generally are constructed without much regard to the strict principles which can alone secure economy. The indicator and its use are almost unknown in the States. This little instrument affords the only known means of ascertaining the exact duty performed by steam within a cylinder; and in its absence, we can only form a vague idea of the really useful effect produced by the combustion of fuel. A constant habit of observation, and a thorough practical acquaintance with a large number of steam engines of different constructions, working under varying conditions, can alone give that knowledge which will permit us to build engines capable of doing a large duty with little coal. This knowledge is tolerably well-diffused throughout the workshops of Great Britain; the young engineer being generally afforded every opportunity for testing the actual performance of engines by the aid of the indicator. More can be learned in this way in half-an-hour than can be derived from theoretical instruction, however good, in a year; and we cannot expect men who are almost, or altogether, unacquainted with the nature and properties of the instrument, to turn out first-class engines. It is very probable that we shall be accused of attaching an undue importance to so small a thing. Those who have studied the subject will take a different view of the matter, and will, without doubt, endorse a statement which will bear repetition—namely, that, without the use of the indicator, there can be no real knowledge of what does or does not constitute a good engine. Until its use extends in America, we do not look for much improvement in steam engineering. Although our Western friends are somewhat lax in their practice elsewhere, we did not expect that their navy would afford such precedents as Mr. Isherwood's evidence indicates—evidence, too, which comes upon us with all the force which can be conferred by an oath, and all the strength which can be derived from the high position of the swearer. Mr. Isherwood has, we fear, led many a feeble mind astray. The engineers of Washington possess a great deal of common sense, however Mr. Mattingly's friends sent to New York for Mr. Dickerson, an adept in the engineering, and a man learned in the law as well—a combination of professional attainments all too rare in England. Mr. Dickerson's powers of cross-examination threw a new light on the subject, from which it appears that the "red tapeism" is all-powerful in the Naval Department of President Lincoln's Administration. To a final suggestion, that the question should be fairly put to the test on the river Potomac, in presence of judge and jury, the defendants turned a deaf ear. The experiment was made, notwithstanding, on board the steamship "Collyer;" when it turned out that with 600 lbs. of coal an hour, she could run at the rate of twenty-seven revolutions a minute with expansion, and that but twenty a minute could be got without expansion, and with 700 lbs. an hour. This evidence was deemed so conclusive that a verdict was at once given for the complainant.

It is not difficult to imagine what English engineers will think of all this. Mr. Isherwood's evidence is opposed to all theory and all practice, that, did he hold a different position, or were he less talented, his word would simply be passed over with a smile, or, perchance, a hearty laugh. The value of expansion, properly carried out, is pretty well understood among us; and it would be unfair to deny that many American engineers attach a proper importance to the principle as well. When the chief engineer of a great navy gives utterance to sentiments and opinions so widely opposed to those held by other experts, a natural curiosity is experienced to know where he got them. We will try and enlighten our readers.

In the early part of the year 1860, the United States Navy Department, in the plenitude of its wisdom, deemed it right that certain experiments should be made to determine the relative economy of using steam with different measures of expansion. Not content with the evidence afforded them by the practical operations of the Cornish pumping engine, the locomotives on the New York railroads, or the marine engines in use on the rivers and seas of the Western Continent, the gentlemen of the Department resolved on instituting thorough experimental research into the whole subject, and with this object in view, they appointed Chief Engineer Isherwood and three other engineers, as a committee, to make the experiments. As very valuable results were anticipated, it is natural that we should suppose that more than one engine was reported on; that the best engines in the navy would be selected, and the necessary data derived from the performance of the engines tried, at sea, under the ordinary conditions of working first, and, subsequently, under such extraordinary conditions as seemed best adapted to the attainment of the end in view. Such a conclusion is not more natural than erroneous. But one steamer, the "Michigan," was selected. At the time of the experiments, February, 1860, she was out of commission, frozen up in Lake Erie; the hull had been recently repaired, and the old boilers replaced with new ones, constructed on Martin's patent, with vertical water tubes, instead of horizontal flues over the furnaces. The engines were two in number, of a form not much known in England; the cylinders, 36 ins. in diameter, and having a stroke of 8 ft., lying in an inclined position, and at an angle of 28 deg. with the keel. Not content with confining the experiment to the narrow basis afforded by a single steamboat, Mr. Isherwood and his colleagues still further restricted it, by retaining only one of the two cylinders in use; the other being uncoupled from the crank-shaft, and put out of communication with the boilers. The valves were of the ordinary double beat poppet kind, habitually used in American paddle engines. The steam valves were made to act as expansion valves, by means of an arrangement, known as "Sickles' cut-off." The distance between the boiler and the engine was 25 ft.; and the total length of the steam-pipe over 17 ins. in diameter, exposed to the refrigerating influence of the atmosphere, was 30 ft.; and the pipes being set on a slight incline, the water of condensation naturally found its way into the cylinder. The steam-pipes were clothed with felt and wood lagging. The heads of the cylinders, the valve chest, and the long nozzles, peculiar to this form of engine, were without covering of any kind. It is needless to enter into any minute detail of the experiments. We have already alluded to Mr. Isherwood's talents, and he spared no trouble to obtain exact results from the engine under trial. The vessel was moored fast, the ice cut away from about the floats, and the whole power of the engine exerted in paddling the water backwards. Each experiment lasted 72 consecutive hours, during which the engine was neither slowed nor stopped. We have dwelt on Mr. Isherwood's proceedings in this matter at some length, because very little about them is known in this country that we are aware of, although they have attracted a great deal of attention in the States already, and are likely to attract very much more. Let us hasten to the results. With the steam cut off at $\frac{11}{12}$ths of the stroke, the consumption of coal amounted to 4·847 lbs. per hour; cut off at $\frac{7}{10}$ths, to 4·324 lbs.; at $\frac{4}{9}$ths, to 3·725 lbs.; at $\frac{3}{10}$ths, to 3·677 lbs.: at $\frac{1}{4}$th, to 3·428 lbs.; at $\frac{1}{6}$th, to 3·801 lbs.; and at $\frac{4}{45}$ths, to 4·281 lbs. From these results we find that the gain by expansion is decided up to a cut-off at $\frac{1}{4}$th of the stroke, and that there is no advantage to be derived from expansion afterwards, *with the particular engine under consideration.* The results perfectly satisfied Mr. Isherwood. He prepared a very elaborate report—a model of its kind—in the course of which he attempts with much plausibility, to upset Marriott's law as applied to the expansion of steam, and laid it before the Hon. Isaac Toucey, Secretary of the Navy. As a result, it now appears, from the Washington investigation, that the engines for the American Navy have been, almost from that day forth, constructed without any regard to the carrying out of the principles of expansion. The crop thus sown will be reaped ere long in diminished speed, and increased coal bills.

If the engine of the "Michigan" had been specially designed to prove that the advantages of expansion were a delusion, it could not possibly have been better adapted to that end. Mr. Isherwood is a careful and, we presume, a conscientious experimenter; but all his conclusions in this case are vitiated by being drawn from false premises. He has recently published a large and handsome volume,* to which we may have occasion to refer again, a large portion of which is taken up with a detailed account of these experiments and their analysis. It required little penetration to see where the author has lamentably failed to make out his case. No provision whatever, either by superheating or otherwise, was made to prevent condensation in the cylinder of the "Michigan's" engine. The length of the stroke was 8 ft.;

"* Experimental Researches in Steam Engineering," by B. F. Isherwood, Chief Engineer U. S. Navy, Hamilton Brothers, Philadelphia; Ballier Brothers, New York.

the number of double strokes per minute, with the steam at full pressure, was a little over 20; with the steam cut off at $\frac{1}{4}$th, rather less than 14. The steam had to traverse some 30 ft. of steam pipe, lightly clothed, in an atmosphere only raised above 29 deg. by the heat abstracted from the engine and boilers, to be subsequently isolated for a space of three or four seconds at a time within a cylinder, lagged, it is true, at the sides, but wholly exposed to the atmosphere at the ends. The cylinder was 3 ft. in diameter, the area of each lid was over 7 square feet. It requires no very abstruse calculation to point out the value of this area as a refrigerator during each tedious stroke. Then, we have the valve chests and nozzles all aiding the same work. Nor is this all. Mr. Isherwood, on his own showing, proves that the loss due to clearance and nozzle space in the "Michigan's" engine amounted to a positive reduction of average pressure equal to 12 per cent. when the steam was cut off at $\frac{1}{6}$th of the stroke, and to nearly 8 per cent. when the cut-off took place at $\frac{1}{4}$th of the stroke. Some men are so mentally constituted that they only regard any fact from one point of view. Instead of gathering from his experience that it was advisable that clearance of all kinds, should be reduced to a lower limit than that found in the particular engine experimented on, Mr. Isherwood, with a strange perversity, takes this very loss of pressure as evidence that expansion is wasteful in practice, and should not be employed! Our readers can form their own estimate of the value of conclusions so arrived at.

It is doubtful if the history of nations can show a more astounding instance of folly in a case where the safety of a country was involved, and that country actually at war, than that presented by the recent proceeding of the Bureau of American Marine. On the evidence afforded by series of experiments on a single engine in an old and small steamer on an inland lake, conducted by four engineers, far removed from supervision of any kind; the engine being, on the showing of those most interested in proving the reverse, totally unsuited in every respect for the application of expansion—the piston moving at but 176 ft. per minute, and the steam not superheated in the slightest degree—those in authority have determined, in their wisdom, to set aside all the teaching of the last fifty years. They have wilfully shut their eyes to the work of improvement going on daily in England and France, and have, with a temerity almost without a parallel, staked the future of a great navy and enormous sum of money on the truthfulness of a single obscure experiment, bearing but a remote analogy in its conditions to those under which steam should properly be employed. It is as though the American engineering world had retrograded the third part of a century. We shall expect, next, to hear war steamers denounced, and the old liner pronounced just the thing to defend the interests of the people, or push on a war of aggression on distant shores. Americans are very clever. They have taught us many things. This time, in the attempt to teach the world something new, they have overshot the mark, and only repeated an old story in our ears—the story of human folly.

On the Effects of Surface Condensers on Steam Boilers.

By Mr. James Jack, of Liverpool.

From Newton's London Journal of Arts, February, 1864.

Some three or four years ago condensation of the exhaust steam from marine engines by surface contact was looked upon as an experiment, and few steamship owners could be induced to try this system, which was new to them: and notwithstanding the development and complete success of tubular surface condensers, both in the merchant ships and in the navy, more than a quarter of a century ago, and the reduction thereby effected in the consumption of fuel, the objection was raised that surface condensation could not have the advantages alleged, or it would never have been discontinued. At the present time, however, it is no longer an experiment, but a reality; for not only have surface condensers been largely introduced with new engines, but in many cases injection condensers have been taken out of working vessels, and surface condensers put in their place. As the writer's firm has constructed and fixed a considerable number of surface condensers during the last three or four years, and as certain actions have been found to take place on the tubes and plates of the boilers with these surface condensers, of such a character that the full advantages of the use of distilled water could not with impunity be obtained, it is the purpose of the present paper to give the particulars of these effects, and thus to induce discussion upon the subject, and elicit information which will enable the great advantages in the saving of fuel resulting from the employment of surface condensers to be appreciated. As the boilers, where surface condensers are used, are insidiously and rapidly acted on, the danger of delay and of accident from explosion is thereby greatly increased,—rendering the question one of serious importance.

Surface condensation may be considered an old idea; for Savery adopted the principle in 1690, in what may be called the first steam engine—having applied cold water to the outside of the cylinder; and even Watt devoted much time to it, although he afterwards finally abandoned it—stating that the objection of the size and expense of the tubular condenser for large engines made him resolve to sacrifice part of the power to convenience, and to employ large pumps. It was about 1832 that surface condensation became practically carried out. Mr. Samuel Hall, having seen its advantages, constructed a surface condenser, consisting of a casing containing a number of small tubes; and succeeded in overcoming the difficulty of making a tight joint between the water and steam spaces, which should allow for the expansion and contraction of the tubes. These condensers were introduced into both naval and mercantile ships; but, although attended with considerable advantages, they ultimately fell into disuse. Several modifications have since been proposed with different arrangements and constructions of the tubes, with the water inside instead of outside the tubes, or with a combined jet and surface condensation. There was a difficulty at first in obtaining some effective packing for the ends of the tubes: but after the long experience of India-rubber for the purpose, it may now be pronounced a success; for out of many thousands of joints which the writer's firm have made, not a single case of leakage has occurred.

Surface condensation is a process by which both the sensible and latent heat of the steam are conveyed away; and although the adoption of any new system is necessarily slow, the writer does not doubt but that surface condensation will ultimately entirely supersede the jet. There are evils, however, which require remedying before surface condensation can be universally adopted for steamships—not in the condenser itself, but in the effects produced on the boilers by distilled water or something contained in it resulting from surface condensation. The writer will accordingly refer, first, to the effects of surface condensation on the boilers, and, secondly, to the probable cause of this destructive action.

A number of marine boilers which have come under the writer's observation, and may for convenience be distinguished by the letter A, had been in use for longer or shorter periods, none less than six months, supplying steam to engines having injection condensers. Salt water had been used for feeding the boilers, and a considerable incrustation had consequently taken place. Without going to the trouble and expense of cleaning these boilers, they were sent to sea immediately after the surface condensers were fitted in. Everything went on satisfactorily, and on returning from the first voyage, the boilers were examined. It was found that the greater portion of the incrustation had fallen off, and that the surfaces of the boilers—that is the inner surfaces—were in very good condition for transmitting heat; showing that the adoption of distilled water for feed had been advantageous. Boilers of this class have uniformly had the incrustation nearly all removed by the action arising from the use of the surface condensers during the first voyage, and in every case the surfaces of the boilers have been found in good condition and still remain so, some of them now for as long a term as four years. Indeed surface condensation has here been in every sense a decided success.

The appearance of the inside of the boilers, however, was not that of clean iron. The surface seemed to be impregnated with some greyish matter, or to be altered in its chemical nature. That this impregnation or alteration of the surface prevented, and still prevents, injurious action on the metal, will be gathered from a description of another lot of marine boilers, distinguished by the letter B, which were in all cases new boilers—sometimes with new engines and surface condensers, in other instances with old engines and new surface condensers. In port, before starting, a number of these boilers were filled with fresh water, while another number were filled with salt water. An examination after the first voyage, during which only distilled water had been used for feeding the boilers, showed the following effects, which were increased in every subsequent voyage, until the practice was adopted of feeding with, say, from one-sixth to one-tenth of salt water. First, both above and below the water-line the surfaces of the plates, tubes, and rivets were covered with a deposit resembling hydrated oxide of iron, which, when the water was evaporated, was in the state of a fine impalpable brownish colored powder. This deposit was thickest above the water-line, sometimes averaging $\frac{3}{4}$-inch thick. When the boilers were emptied, a thick slimy deposit adhered all over the inside, an analysis of which showed that it consisted of—

Oxide of Iron	77·50
Moisture	19·75
Grease	0·85
Sulphate of Lime	0·80
Oxide of Copper	0·60
Traces of Alumina and Chloride of Sodium and Magnesium Loss	0·50
	100·00

Secondly, underneath this deposit the plates and tubes were found to be eaten into, indented, or "pitted." The indentations varied in diameter, from the smallest speck to $\frac{5}{8}$-inch, and in depth from the merest impression to the entire thickness of the plates or tubes. And although they were formed all over the boilers, they were most frequently found and were most numerous just over the fireplaces, and in those parts immediately in connexion with the greatest heat. In some of these parts, the surface was entirely covered with the indentations; while in other parts, as much as a square foot of plate, although subjected to the greatest heat, was free from them. The plates and, tubes in all cases, have been of the best iron, and by good makers; and the "pittings" occur in what looks like iron of good quality with a good fibre; no slag or cinder being perceptible. So destructive was this pitting in boilers using the same water over and over again, that in one instance, the tubes of new boilers were actually eaten through at the end of two or three voyages, extending over only a few months altogether, and it became necessary to put in new tubes, and to use a portion of salt water for feed to keep up an incrustation, so that the boilers should not be acted upon. If the iron of the boilers had been all of one make, it would naturally have been concluded that the pitting was due to the quality of the iron; but as the iron of different boilers had been obtained from different makers from time to time, the quality of the iron could not be blamed.

The presence in the boiler of a soft metal—such as copper from the condenser tubes—it was considered, would induce a galvanic action such as might affect the iron in some way. But the analysis which was made of the deposit scraped from the boiler, shows that there was scarcely a trace of any foreign metal there. Indeed, it might have been concluded, that a soft metal could not be present; for the tubes of the condenser and the copper pipes were all in a perfect condition. Even at the joints, made tight by India-rubber, hardened by vulcanizing, there was scarcely a speck of corrosion.

A search was then made to ascertain whether the gluey deposit was present that arises from the decomposition of the tallow and oil used for lubrication; as the writer had frequently heard that such a deposit took place in boilers where Hall's surface condensers were used. For the purpose of ascertaining this, the mud cocks of a vessel were not opened for some time before arriving in port; and the fires were then put out on arrival, and the mud discharged, when the only substance found was the watery-brownish deposit before referred to. The deposit remaining in the bottom of the boiler was carefully examined, but here again there was only the same deposit. As it was believed that the lubricating material carried into the boilers with the feed might, by continued subjection to heat, form an acid capable of producing the effects observed, the kind of lubricating material employed was noticed, in order to ascertain whether animal or vegetable oils acted most injuriously; but it was found that the action went on as much with the one oil as with the other. In case, however, a fat acid, formed as already mentioned, might be the cause, pieces of chalk were put into the boilers, and from time to time fresh pieces were added; carbonate of soda was also mixed with the feed water in regular doses: but all to no purpose, the action went on, getting worse and worse.

No alternative was therefore left, nor is there at present any other, as far as the writer has been able to learn, but to feed the boilers with a portion of salt water sufficient to keep a thin incrustation over the surface of the iron. It was suggested that the deposit was nothing else than rust or oxide of iron, and that it was formed by the chlorine present in the small proportion of salt water, which would combine with the iron to form chloride of iron; and this being readily decomposed by oxygen, oxide of iron would result. The difficulty here, however, was to know whence the oxygen was obtained; for the quantity of air entering with the feed water must have been very small indeed. It was also suggested that hydrochloric acid might be present from the small quantity of sea water that may have found its way into the boilers; but then the difficulty was to know where a quantity of the acid was to come from sufficient to act over such an extended surface, and so rapidly as the results showed.

It was found, however, by Mr. Rollo, one of the writer's partners, that in a pair of boilers at a sugar refinery, there was the same brownish deposit adhering all over the boilers, and those parts subjected to the greatest heat were "pitted" in precisely the same manner as the second lot of marine boilers previously designated by the letter B. Exactly the same effects were being produced. These boilers were supplied with the same water over and over again, a small quantity of fresh water being added from time to time to make up for the loss. As the steam was passed only through iron pipes for melting the sugar, the damage to the boilers could not result either from the steam coming in contact with a soft metal, or from any lubricating material. The boilers were of the Cornish construction, with one flue, and were worked at about the same pressure as the marine boilers B already referred to, say 20 lbs. per square inch pressure. A pair of boilers, of exactly the same construction, placed alongside the first pair and working at about the same pressure, but fed with water which had not been distilled, were then examined, to learn what state they were in. But, although put in about the same time as the two first examined, these boilers were found in good condition and likely to last for years, as there was not a sign of corrosion or "pitting;" whereas the two boilers working with distilled water had to be repaired.

The practical knowledge thus acquired necessarily led to the conclusion that the distilled water itself was the cause of the corrosion, instead of any galvanic action or any fatty acid. In reference to the question whether distilled water has any particular action on metals, the chemist Berthier found that nodular protuberances deposited on iron pipes containing distilled water consisted of 21 per cent. of protoxide of iron, 58 per cent. of peroxide of iron, 5 per cent. of carbonic acid, 14½ per cent. of water, and 1⅓ per cent. of silica. The iron pipes contained also a perverulent substance, which could be produced at pleasure with distilled water to which a trace of carbonate of soda and common salt had been added, but not with an addition of caustic alkali. Distilled water is known to act powerfully on lead, and this action is attributed by Dr. Clark to the remarkable property that distilled water has, as compared with ordinary water, of dissolving free carbonic acid.

The writer does not presume to state confidently that distilled water is really itself the active destroyer of iron boilers; but, from the observations that have now been referred to, and the information he has been able to obtain, he thinks there is sufficient evidence that distilled water is, if not the sole cause, at least an active agent in producing the corrosive effects that have been described. If this suggestion should lead to the remedy of the evils that have been experienced where distilled water alone has been used, another difficulty will have been overcome towards the complete introduction of surface condensers.

Mr. J. Ramsbottom said he had been much interested in the paper just read, and the conclusions arrived at coincided with the results of his own experience with stationary and locomotive engines, not with distilled water, indeed, but with peat water, which was almost as pure as distilled water. He had been informed that the boiler of the winding engine which formerly worked the Woodhead Tunnel on the Sheffield and Manchester line was corroded through in a very short time, being fed exclusively with the peat water from the Yorkshire moors; and on putting down a second boiler in its place, the precaution was taken of mixing with the peat water a portion of water from one of the shafts, the result of which was that the corrosion was almost stopped, but an incrustation of deposit was then formed inside the boiler. In the same range of hills he found that the locomotives working the Huddersfield and Manchester line, with almost pure water, had their boilers corroded in the same way; and he had now adopted the plan of putting a portion of carbonate of lime in the tanks, to prevent the corrosion.

Mr. C. Markham had also found the use of nearly pure water injurious to boilers, and the evil effects continued until the interior of the boiler became sufficiently coated with calcareous matter to prevent the corrosion going on. The effect produced by using pure water in boilers that had previously been worked with water containing a great quantity of lime was very remarkable: some of the locomotives on the Midland Railway were supplied with water that contained much lime, causing a great deposit in the boilers; and when any of these engines were sent down to Sheffield, where the water came from the moors and was very pure, the effect of the change was that the deposit was speedily removed in large masses; and he had seen a barrowful of the calcareous matter removed and washed out of a locomotive boiler after it had worked for a single day with the Sheffield water. So long, however, as an incrustation of lime remained on the plates, it had the advantage of protecting them from any injury by corrosion from purer water.

Mr. J. McF. Gray thought the "pitting" observed in the case of locomotive boilers working with pure water was due to the brass tubes used in the boilers: for, on examining the pitted plates by the microscope, he had detected a minute speck of brass in the centre of so many of the "pits" as he thought would warrant the conclusion that this was the cause of all the pitting, in consequence of the galvanic action produced by the particles of brass. The corrosion of the iron had accordingly been prevented in those cases by suspending a piece of zinc among the boiler tubes, which neutralized the action of the brass. The pitting had not been so extensive in the hottest plates of the boilers as in other parts, because at the hottest parts the ebullition of the water was more violent, so that there was less facility for the particles of brass becoming deposited there upon the plates to cause the corrosive action. From the paper it appeared not to be expected that distilled water would be able to be used successfully in boilers: but if the difficulty arose merely from the water used being distilled water, it might easily be got over by aerating the water, since the only difference in distilled water was, that it was not charged with air like ordinary water. The real objection, he considered, to surface condensation was that the same water was then used over and over again without change; and he thought this must be the reason of the corrosion observed in the boilers of sugar refineries, the steam being condensed over the surface of the evaporating pans, whereby the water became in course of time impregnated with particles of brass or copper from the pans and pipes, gradually accumulating in the unchanged water. In the sugar plantations of the West Indies, where there was a difficulty in getting fresh water, and the same had to be used over and over again, the water was found to get sour, as it was termed, and became more and more injurious to iron, the longer it was used without change. He therefore thought the corrosion observed was to be attributed to the specks of brass carried into the boiler by the water where there was surface condensation of the steam.

Mr. D. Rollo observed that the object of the paper was not to discourage the use of surface condensation, but to point out the difficulties that had been met with in its application, with a view to ascertaining how they could be removed. With regard to the boilers at the sugar refinery that had been referred to, the corrosion could not, he thought, have been caused in that case by the presence of brass particles in the water; for the steam passed only between the two cast iron plates of the evaporating pans, and when condensed, returned again to the boiler, without coming in contact with any brass at all in the apparatus, the leakage being made up by the addition of a little fresh water. There were four boilers, all working together,—two fed with the distilled water returned from the evaporating pans and two with fresh water: the two latter, at the time of examination, were found in perfect condition after four years work, having only a large deposit or scale of lime over the surface of the iron; while the other two fed with the distilled water, after the same time of working, had become quite unsafe from corrosion, and had to be very extensively repaired. They had tried various remedies for the evil; suspending zinc plates in the boilers, but with what result had not yet been ascertained; cast iron pipes instead of copper pipes for conveying the steam; and tinned tubes, zinc, and galvanized-iron tubes in the condensers instead of brass. Block tin tubes had also been tried, but were found not to have strength enough to support their own weight when placed horizontally. All these trials therefore still left the question of the cause of corrosion undecided: but he thought it could not be the action of copper, because the analysis that had been made of the deposit found in the boilers using surface condensers showed a very small proportion of copper, the main metallic ingredient being iron, and there was not sufficient copper collected from the boilers in the voyage of a steamship to account for such an extent of corrosion as was met with. Where much grease, however, was used in the engine, he had seen the inside of a boiler present an appearance which he thought rendered it just possible that the fatty matter or acids contained in the grease had something to do with deteriorating the quality of the water and causing its corrosive action.

Proceedings Mech. Eng. Society, August 2, 1863.

Steel Ships.

From the Journal of the Society of Arts, No. 576.

Two large ships, built of steel plates, were recently launched in the Mersey. Though some small vessels have been built of the same material, this is the first instance in which steel has been used for ocean ships. The steel now manufactured for shipbuilding purposes is said to have an advantage over iron, in being more ductile and malleable, as well as stronger and lighter. These qualities bring with them, it is also said, greater economy in building and increased capacity, both most important considerations. Mr. Jones, one of the builders, states that in the *Formby*, one of the newly-launched vessels, of 1276 tons burden, the weight of steel used is 500 tons, whereas, if she had been constructed of iron, 800 tons of that metal would have been required. In a vessel like the *Warrior*, he declared that by using steel greater strength might be obtained with a saving of one-half in the weight of metal. Mr. Reed, Constructor of the Navy, made a special journey from London to attend the launch and examine the ships. He remarked that merchant ships can be built to test a principle, when war ships cannot, as the former can be examined and repaired annually, while the latter are sent abroad for periods of three or four years. He perfectly agreed with what had been said of the importance of steel for the construction of small ships, and stated that the Government took great interest in the question of employing steel as a material for shipbuilding.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

From the Journal of the Society of Arts, No. 554.

(Report of the Chief Engineer, April 28th, 1863.)

During the past month there have been examined 288 engines and 406 boilers. Of the latter, 6 have been examined specially, 11 internally, 79 thoroughly, and 310 externally; in addition to which, 2 have been tested by hydraulic pressure. The following defects have been found in the boilers examined:—Fracture, 5 (one dangerous); corrosion, 30 (one dangerous); safety-valve out of order, 1; water-gauges, ditto, 8; pressure gauges, ditto, 9; feed apparatus, ditto, 1; blow-out taps, ditto, 32; furnaces out of shape, 4; over-pressure, 4; blistered plates, 4.—Total, 98 (two dangerous). Boilers without glass water-gauges, 6; without blow-out taps, 13; without back pressure-valves, 21.

Some explosions occur at too great a distance from Manchester to admit of a personal investigation, and of these little or no reliable information can be obtained at the time. Of one such which took place last year, and of which the fact of its occurrence only was therefore recorded in the reports, there has since been, through the kindness of the engineer who investigated the explosion immediately after it happened, full information with regard to it, as well as a drawing of the boiler after rupture, both of which are placed at the disposal of the Association. From these it appears that the boiler in question was of plain Cornish construction, and that the explosion resulted from collapse of the internal flue tube.

The explosion was attended with fatal consequences, the rush of steam and water from the flue blowing down the end wall of the boiler-house, abutting against a public thoroughfare, in which the workmen of the establishment, most unfortunately, were assembling around the gates of the premises, a few minutes before bell time. The steam, water, and bricks were all scattered amongst the group, and five lives sacrificed.

The length of the boiler was 26 feet; the diameter of the shell, 7 feet; that of the internal flue, 4 feet; and the thickness of the plates in both, three-eighths nominally, while at the centre of the flue it was found, after the explosion, to have been but little more than one-quarter. The boiler had been purchased second-hand, while the workmanship is reported to have been inferior, and the flue tube, though supposed to be circular, to have proved actually oval at the middle of its length. This flue was not strengthened by flanged seams or hoops, while the working pressure was 50 lbs. to the square inch.

Under these circumstances it is clear that the simple weakness of the flue was the cause of the explosion, which must, therefore, be added to the already long list of those which could have been prevented by the simple precaution of strengthening the flue, either with flanged seams or hoops, while the weakness could not have escaped detection on competent inspection, or practical exposure on the application of the hydraulic test.

No. 6 explosion, like the preceding one, occurred at a considerable distance from Manchester, and although it took place last month, was only reported within the last few days; and there has as yet been no opportunity of obtaining further particulars with regard to it than that the boiler was of plain cylindrical, egg-ended construction, externally fired, and was thrown by the explosion to a considerable distance from its original seat, which is always the case with this description of boiler.

Neither of the two boilers just referred to were under the inspection of this Association.

Three explosions have occurred during the past month to boilers not under the inspection of the Association, by which nine persons were killed, and four others injured. All of these explosions occurred so far from Manchester that only one of them—viz: No. 8, which was by far the most important—had been personally investigated.

The following is the monthly tabular statement:—

Tabular Statement of Explosions from March 28, 1863, to April 24, 1863, inclusive.

Index No.	Date.	General Description of Boiler.	Persons killed.	Persons injured.	Total.
No. 7.	April 7.	Plain cylindrical Externally fired, .	0	0	0
No. 8.	April 8.	Four cylindrical egg-ended Externally fired, . . . One ordinary single flue or "Cornish" Internally fired, . . (These five boilers all exploded simultaneously.) . .	9	4	13
No. 9.	"	Plain cylindrical Externally fired, .	0	0	0
Total,	.	. . .	9	4	13

No. 8. *Explosion.*—This explosion occurred at an iron-works, and is one of the most remarkable that have come under the notice of this Association—five boilers, working side by side, having in this instance exploded simultaneously, the shells of all of them being rent asunder, and thrown to a considerable distance from their original brick-work setting, which was completely destroyed, and reduced to a heap of ruins. The fragments and boiler fittings were shot in every direction, many of them falling through the roofs of the adjoining buildings, and one through that of the dwelling-house of the proprietor. Two of the

boilers were thrown over a tramway, and landed in a ploughed field beyond; one of them raking the rails in its course, cutting them completely through at the solid metal, and tearing up the roadway, a piece of one of the rails, that had been shorn off, being carried away to a distance with the boiler.

The five boilers were ranged side by side, all of them being connectted, both by the steam-pipe and feed-pipe. Their direction was very nearly north and south, the furnaces being at the south end. No. 1 boiler, commencing at the west, was of Cornish construction and internally-fired, while the remaining four were cylindrical, with egg-ends, and externally-fired. The length of all the boilers was about 25 feet, while the diameter was 6 feet in No. 1, and 5 feet in the remaining four, the thickness of the plates being seven-sixteenths in the former, and three-eighths in the latter. The boilers were each fitted with one feed-back pressure-valve and feed stop-valve combined, one glass water-gauge, one alarm low-water steam whistle, worked by a float fixed inside the boiler, and one lever safety-valve, of 4 inches diameter, in addition to a Bourdon's pressure-gauge fixed on the steam-pipe at some distance from the boilers. The safety-valve admitted of nearly 60 pounds per square inch, but it was stated that 45 pounds had been the limit at which the boilers had been worked, since the weight had always been placed at some distance from the end of the lever. Even the higher pressure, however, would not have been excessive for boilers of such dimensions, as far as the cylindrical shells are concerned.

The rents were extremely complicated by the effects of the explosion, and although highly important to do so, it is in these cases difficult to determine which are the rents from which the explosion sprung, and which are those that resulted from it; or otherwise, which rents are the primary ones, and which the secondary ones. It appeared, however, that all the egg-ended boilers had rent in two, transversely, one portion flying northward and the other southward, the rent occurring at one of the transverse seams over the furnace, while the larger portion had in every instance flown northwards. In some cases both ends were blown out, and the plates "vandyked" and curled up, the distortion being such as is difficult to describe. These latter effects were due to the flight of the parts and the consequent shock on grounding, and may be considered as secondary, while it is clear, from the direction in which the parts had flown, as well as from the character of the rents themselves, that the primary ones, from which the explosion had sprung, were those at the transverse seams over the furnace.

The Cornish boiler was also rent transversely, at about the middle of its length, both through the shell and flue tube, the end furthest from the furnace being demolished, the shell being opened out, ripped spirally, and the end plate torn off. The other half of the boiler was comparatively uninjured, the whole, though thrown from its original seating, holding together, and the furnace retaining its cylindrical shape.

There is no reason for attributing the explosion either to a deficiency in the number of the fittings, to unwise or excessive pressure, or to shortness of water, but to the weakness of the transverse seams over the furnace,—to which all externally-fired boilers are found to be liable,—aggravated in the present instance by the use of sedimentary water and the mode of introducing it, by which it was injected directly upon the plates at the bottom of the boiler, and immediately over the fire.

There still remains to be explained the fact of the whole series of five boilers having exploded simultaneously.

The percussive force of water, as an important element in producing the destructive effects in steam boiler explosions, has lately excited considerable attention, and is a subject of much interest. The view is as follows:—The temperature of water in a steam-boiler working at 50 pounds pressure, is 300°, which is 88° above the boiling point at atmospheric pressure. On the occurrence of a rent above the waterline, or the rending off of a steam dome, the blowing-off of the man-hole cover, or the fracture of a steam-pipe, &c., the steam above the surface of the water would escape and remove the pressure from it. The 88° of free heat, with which the whole body of water is charged, over and above that necessary for maintaining it at the boiling point at atmospheric pressure, would instantly flash a considerable portion of it into steam, while the globules, not being generated at the surface merely but throughout the whole mass, would blow up the water with considerable force, converting it into a projectile, having a velocity equal to that due to the pressure. This would give it a destructive force far greater than that of the simple statical pressure of the steam; just as in the injector the stream of water acquires a force of penetration that renders it superior to the pressure of the steam that imparted it, and which enables it to enter a boiler working at a higher pressure than the one supplying the injector. It is thought, by its advocates, that on this principle it is possible that an explosion might happen, without either the existence of any previous weakness of the boiler, the occurrence of any primary rent, or excessive pressure, but simply from the sudden removal of the steam from the surface of the water, when it would be violently projected, as just explained, against the upper part of the shell. It has been proposed to submit this to the test of experiment, and to accomplish the instantaneous clearance of the steam space, either by condensation, effected by the introduction of a jet of cold water, or by the opening of a large valve. Such an investigation would certainly be most interesting and useful, while the result would be highly prized by the engineering world. Of the fact of the sudden liberation of the steam throwing up the water there can be no question, since that is already a matter of experience; whether, however, the entire steam space can be so instantaneously cleared as to allow sufficient velocity of the water being attained—on the accomplishment of which the force of impact depends—appears to me a matter of question, and I cannot but think that steam would be generated with sufficient rapidity on the surface of the water, to retard the velocity of the mass, as well as to form a cushion which would soften the blow against the shell of the boiler. This is a nice point, and must await the issue of the experiment for determination. I have for some time since, in examining exploded boilers, carefully searched for indications of this action, and it appeared at first that there were manifestations of it in the explosion under consideration. For instance, the feed water had been introduced, as previously stated, at the top of the boiler, the feed stop-valve being bolted to the side of the steam dome, and the water carried by the feed-pipe through the steam space. The failure of this feed-pipe would have allowed the water to play amongst the steam, and thus produce the very condensation desired in the proposed experiment just mentioned; added to which, the steam domes were torn off from the shells, as if by upward impact, and blown in a different direction to the boilers themselves. The feed water, however, proved to have been heated by the exhaust steam from the engine, and thus would have produced but tardy condensation; and even if the flight of one boiler, on the rending of the transverse seams over the furnace, had carried away the steam pipe, or rent off the domes of the others, which is highly probable, and thus by suddenly relieving the steam-pressure, set up this percussive action—still, the force of the impact, which must have been upward, can hardly be supposed to have developed the transverse rents previously described, or to have thrown one-half of the boiler to the south and the other to the north. With a full appreciation of the value of the impact theory, it is not thought that it implies in the present instance, as having produced primary rents, important as it may have been in developing secondary ones, but every opportunity will be taken of watching for its manifestation, and communicating the results to the members.

The cause of the simultaneous explosion is thought to have been as follows:—A single externally-fired egg-ended boiler, say No. 3, rent at one of the transverse seams over the furnace in the first instance. The escape of steam and water from the bottom of the boiler then lifted the remaining ones, and threw them up in the air several feet high, blowing down at the same time the brickwork seating, so that the boilers on coming again to the ground, fell upon a loose irregular bed, and all became so strained that each rent and exploded in turn. That the percussive action of the steam was sufficient to have done this, is illustrated by the fact, that one of the cast iron rolls from the mill was lifted by it at the time of the explosion to the height of several feet.

A reference may be of assistance, to a very similar explosion which occurred last year, the particulars being given in the monthly report for March, 1862. In that instance there were four boilers set side by side, and connected together; three of these were egg-ended and externally-fired, while the fourth was a Cornish boiler. The three former were at work and all exploded simultaneously, while the latter, not having steam up, escaped, but had evidently been lifted some considerable height by the explosion of the others, and on its fall rent at the bottom of the shell transversely, and the flue tube was also dislocated at its connexion with the end plate. The fall had made such serious rents in the shell, that had there been any steam in the boiler at the time, an explosion must inevitably followed. This, it is thought, affords the key to the explanation of the simultaneous explosion of the five boilers under consideration, and which, it is concluded, was due simply to the fracture of a single externally-fired boiler, at the transverse seams over the fire, in the first instance; and to the effect upon the others, of the percussive action of the steam and water escaping therefrom, in the second.

Explosions No. 7 and No. 9 were very similar in character one to the other. Neither of the exploded boilers having been personally examined, only the following scanty particulars have been obtained:—

No. 7 explosion occurred at a colliery to a cylindrical externally-fired boiler, 36 feet long and 5 feet in diameter. This boiler formed one of a series of five, being the second from the engine-house. It was rent by the explosion into four pieces, while the escape of steam and water blew up the boiler seated next to it, which was the centre one of the series, flattening it on one side, reversing it end for end in position, and rolling it over the other boilers, finally depositing it on the ground beyond. It will be seen that the treatment of this boiler, on the rupture of the one alongside of it, corroborates the view given with regard to the cause of the simultaneous explosion of the five boilers referred to above, and it is hoped that fuller particulars may be obtained.

No. 9 explosion occurred at an iron-works, to a cylindrical externally-fired boiler, about 35 feet long and 5 feet diameter, which was the outer boiler of a series of five.

It will be observed that the seven boilers which have exploded during the month have all been of the egg-ended, externally-fired class, with the solitary exception of the Cornish one, which suffered only through the explosion of the others; and it is trusted that the fact of the constantly recurring failure of these externally-fired boilers will at length dispel the false estimate so generally entertained of their safety. The proprietor of the iron-works at which No. 8 explosion occurred, at once adopted the wisest course, and resolved upon having no more of the externally-fired class upon his works, but laying down "Lancashire" boilers in their place; experience having shown him that the plates over a furnace of an internally-fired boiler may fail without moving it off its seat, or causing any loss of life; while with an egg-ended boiler its entire rupture and the flight of the parts, as in No. 7 explosion, is the result.

In another case one of our members, employing several of these externally-fired boilers, has found them to crack at the transverse seams, without warning, for a length of two feet, and consequently determined, as in the previous instance, to condemn them altogether; while to prevent rupture in the meantime, before they can be replaced, contemplates lashing the two ends together with strong longitudinal stays, so as to prevent their flying apart should failure at the transverse seams occur.

On the Behavior of Chloride, Bromide, and Iodide of Silver in Light, and the Theory of Photography. By HERMAN VOGEL.

From the London Chemical News, Nos. 206, and 208.

(Continued from page 127)

The next question entered upon by the author is that of the influence which foreign matters exert on the changes which chloride, iodide, and bromide of silver undergo in the light. Water appears to have no influence on the action of light. In the case of chloride of silver, acetic and dilute sulphuric acid seemed to exert no influence on the changes of color, but nitric acid appeared to delay the coloration. AgCl placed in yellow fuming NO_5 colored slowly, partly in consequence, the author supposes, of some absorption of the chemical rays. Fuming SO_3 completely prevented the coloration; after a week's exposure the AgCl remained perfectly white. AgCl is slightly soluble in fuming SO_3, and forms with it a compound insensible to light. HCl prevents the coloration of AgCl, perhaps in consequence of the formation of a compound on which light has no action. A solution of sulphate of iron also hindered the coloration of AgCl by absorbing the greater part of the chemical rays.

The influence of dilute acids on the coloration of bromide of silver is insignificant. That exposed under nitric acid was much less colored than that exposed for the same time under water. On boiling colored AgBr in NO_5, sp. gr. 1·2, the grey violet mass changed to a uniform yellow grey, but no silver was dissolved.

The influence of acids on the changes of $AgI\beta$ are the most striking. Five glasses were simultaneously exposed containing $AgI\beta$ with nitric acid (sp. gr. 1·2), sulphuric acid $({}_1HO,5SO_3)$, acetic acid, nitric acid, and water. After a few minutes the differences in color were remarkable. The iodide under water appeared of a deeper grey than that under the acids; but that under nitric acid, was still yellow. After an hour's exposure that under the NO_5 still appeared a pure yellow, under SO_3 it was somewhat grey, under acetic and citric acids of a darker shade, but that under water was darkest. After a day's exposure the iodide under NO_5 also became grey.

When NO_5 is poured upon grey AgI, the iodide is quickly restored to its original yellow color, but no silver is dissolved; but some decomposition takes place on long standing. SO_3 also exerts a slight bleaching power.

$AgI\beta$ exposed under a solution of KI did not change in the least. When a solution of KI on grey-colored AgI, the latter turned yellow immediately. In the former experiment iodine was set free in consequence of the decomposition of some KI. The author proved that a solution of KI in water is quickly decomposed on exposure. Paper soaked in solution of KI, and exposed under a negative, gave a yellow picture on a white ground, the former turning blue when a dilute starch solution was applied.

The author comes next to the *influence of nitrate of silver.* In the case of *chloride of silver* exposed in a solution of one part $AgONO_5$ to five parts HO, the action of light seemed to be a little accelerated.

When bromide of silver was exposed with the solution of nitrate it became intensely colored in a few minutes. In diffused light the change was remarkable; but when exposed to direct sunlight the AgBr in a few minutes became of a dark blackish grey with a shade of violet. AgI under the same circumstances, was colored in a few minutes of deep grey with a greenish shade. This deep coloration took place when a dilute solution of $AgONO_5$ was employed. When AgBr and AgI were both exposed together in a solution of $AgONO_5$, the AgBr became more deeply colored than the AgI.

To account for these appearances, the author first exposed simple solutions of nitrate of silver, and satisfied himself that even when pure water was employed some decomposition took place, and silver was separated. He believes that the nitrate of silver is first reduced to the state of suboxide, which is acted on by the free nitric acid and further reduced to metallic silver. It was found that AgBr exposed in the nitrate solution showed the presence of free silver, and the author infers that the reduction of nitrate promotes the reduction of the bromide, and believes the darker coloration to be the consequence of this double action.

In chloride of silver exposed in solution of nitrate the author found that a separation of silver took place.

In the case of AgI exposed in a solution of $AgONO_5$ the author also believes that some reduction takes place. A solution of $NaOS_2O_2$ poured on iodide so exposed leaves behind a small quantity of greyish-black granules, which may be separated by long washing and decantation. They dissolve in NO_5 and the solution gives a precipitate with HCl.

Schnauss ascribes the intense coloration which takes place with AgI under the nitrate to the formation of a compound of $AgI+AgONO_5$, which is peculiarly sensible to light. He found that on boiling AgI in a concentrated solution of the nitrate, silky crystals were deposited, which very quickly blackened in the light. Sutton and Schnauss both suppose the sensibility of AgI to depend upon the presence of a trace of nitrate which cannot be removed by washing. The author himself regards the heightened sensibility of AgI in contact with $AgONO_5$ as the result of a simple contact action. He denies that a trace of nitrate has the effect ascribed to it, and quotes the following experiments to support his position :—

He took paper prepared with AgI/β, and after well washing, left it for five minutes in a solution of common salt (five per cent.). If any trace of nitrate had remained it would then have been converted into chloride. The paper was then again well washed, and afterwards exposed. It behaved in exactly the same way as paper not treated with common salt.

The author then gives the following *resumé* of the results he obtained in his experiments :—

1. Chloride of silver, whether prepared with an excess of silver salt or with excess of HCl or NaCl, is decomposed in the light, becoming colored violet, not black; chlorine being set free, and a subchloride left behind.

2. This decomposition takes place when all moisture is excluded, and under English SO_3 and NO_5.

3. HCl and fuming SO_3 completely prevent the decomposition; English SO_3 and NO_5 only delay it.

4. Sulphate of iron solution also delays the action by absorbing some of the chemcal rays.

5. A solution of $AgONO_5$ hastens a little the coloration of AgCl by light. In this case free silver instead of a subchloride is formed in consequence of the decomposition of the nitrate.

6. Bromide of silver, whether prepared with an excess of silver salt, or of KBr solution, is colored a pale grey violet in the light, giving bromine and leaving a subbromide of silver.

7. This subbromide decomposes in NH_3, giving metallic silver and a solution of AgBr.

8. Nitric acid delays the action of light on AgBr more than on AgCl.

9. By heating the colored AgBr with NO_5 the color is changed to a yellow grey, and no silver is dissolved.

10. Nitrate of silver solution greatly hastens the action of light on AgBr, and in this case not only a subbromide but metallic silver also is produced.

11. AgCl precipitated from an excess of silver solution decomposes sooner in the light than AgBr precipitated under like conditions. AgCl precipitated in excess of NaCl solution, on the contrary, decomposes more slowly than AgBr precipitated by an excess of NaBr.

12. AgI precipitated by an excess of KI solution is not in the least affected by light.

13. AgI precipitated in an excess of silver solution becomes colored a pale grey in the light. No iodine is set free, and any chemical decomposition is in the highest degree improbable.

14. Acids, even diluted, delay the coloration of AgI in the light.

15. Nitric acid, specific gravity 1·2, restores the yellow of darkened iodide, but no silver is dissolved.

16. KI solution instantly turns the deep yellow AgI precipitated in an excess of silver salt to a pale yellow and destroys its sensibility; it also turns the grey colored iodide to a pale yellow.

17. $AgONO_5$ greatly promotes the action of light on AgI.

18. Of the three salts AgCl, AgBr, AgI, when exposed in contact with $AgONO_5$, AgBr becomes most, and AgCl the least colored.

19. Pure solid $AgONO_5$ does not decompose in the light, but a small quantity of silver separates from a solution in the form of small black granules.

20. Solid KI does not decompose in the light, but free iodine is soon separated on the exposure of a solution.

The author next occupies himself with the changes which are produced in the developing process.

What are *the changes produced in the developing process?*

The author starts with a review of the various opinions advanced by Schnauss, Sutton, Hardwich, Monckhoven, Devanne, and others. This we omit, and proceed with the original experiments here detailed.

In the first place he prepared a number of papers, by floating them first on the solutions of iodide of potassium, bromide of potassium, and chloride of sodium, and afterwards on a solution of nitrate of silver containing 8 per cent. of $AgONO_5$. These were well washed and then dried.

The AgCl paper was quite white, the AgBr pale yellow, the AgI dark yellow. Papers so prepared were carefully preserved from the light for some weeks, during which time they were but very slightly colored.

The developer employed was a solution of one gramme of pyrogallic acid, and one gramme of citric acid in 250 grammes of water. Four drops of a 5 per cent. nitrate of silver solution were added to five cubic centimetres of the foregoing solution. The object of the citric acid was to delay the reduction of the silver salt by the pyrogallic acid.

When pure pyrogallic acid is mixed with a silver solution a precipitate is almost immediately produced of granular, pulverulent silver, mixed with a black substance, which may be extracted by means of ammonia. Such a precipitate is obtained on the unexposed as well as the exposed parts of the paper; but when an acid is employed the precipitate appears more slowly, and first on the exposed part, and only later when the decomposition has proceeded to the end does the unexposed part become faintly colored. It is easy to remove the developer before the coloration of the unexposed part takes place.

First experiment. Strips of the chloride, iodide, and bromide of silver paper, prepared as described, and also a strip dipped only in solution of $AgONO_5$ and afterwards washed, were exposed to diffused daylight for one minute, half of the paper being covered. The exposed part of the AgI paper was colored pale grey, the AgBr pale grey violet, the AgCl pale violet.

The developer before mentioned was poured over the papers simultaneously. The exposed parts presently became darker, the AgI soonest, then the AgBr, last the AgCl. The coloration gradually deepened, and in the end the AgI became of a deep black grey, the AgBr a clear brownish grey, and the AgCl a pure brownish yellow shade.

The paper prepared with nitrate of silver only was but faintly colored on the exposed part, but left in the air for some minutes it became brownish, a proof that a trace of silver was left after the washing.

2. The same experiment was repeated, but in place of the developer before mentioned, a solution of pyrogallic acid *without any silver solution*, was poured over the papers. By this the papers were *not colored* even after a minute.

From these experiments it results:—

1. That pyrogallic acid *alone* does *not* effect any change in exposed AgI, AgBr, and AgCl paper.

2. That the haloid salts of silver in the nascent state possess the property of attracting and fixing the separated granular and pulverulent silver.

3. That paper soaked in silver solution only, and even after long washing, retains a trace of silver, and browns in the light, behaving with the developer just like AgCl, AgBr, AgI, paper.

The author afterwards repeated his experiments with papers prepared according to Bunsen and Roscoe's method by complete immersion in the solutions, and ascertained that of the three silver salts, after exposure to light under perfectly similar circumstances, the AgI is most deeply colored by the developer, and the AgCl the least This is remarkable, inasmuch as AgCl is the salt which is most deeply colored by light alone, and undergoes the greatest chemical change; while, on the contrary, AgI undergoes the least visible alteration in the light, and probably suffers no chemical change. It follows that the change occasioned by light, which renders these capable of attracting silver particles, is not parallel with the chemical changes, and, indeed, may be different from them.

For this reason we must assume another action of sun-light besides the chemical. This action, which fits iodide of silver and other bodies to attract and fix particles of silver the author calls the *graphic action.*

The capability of certain bodies after exposure to attract the silver particles (and also mercury particles as in Daguerre's process) he calls the *photographic sensibility.*

The dark coloration which they assume under the developer, the *photographic coloration.*

In opposition to these the author places the *chemical action*, that is, the disposition to chemical decomposition, or composition conferred by light:—

The *photo-chemical sensibility*, that is, the capability of certain compounds of undergoing decomposition in sunlight, and

The *photo-chemical coloration*, that is, the color changes that they suffer.

Thus, we may say, that iodide of silver possesses the most photographic sensibility, chloride of silver the least, while bromide of silver stands between the two.

The author then proceeds to describe the *photographic* behavior of the silver haloids in the presence of other bodies.

1. *Influence of Water.*—AgCl, AgBr, and AgI papers, half of each being wetted with distilled water, were exposed, while still moist, for a minute, and afterwards the developer was poured over. The dry and the moist parts behaved photographically almost alike.

2. *Influence of Acids.*—We may sum up this by saying, that from the author's experiments it follows that *acids weaken or destroy the photographic sensibility.* Papers moistened with nitric acid *after exposure* were quite indifferent to the action of the developer, while the part not moistened darkened, showing that the acid had destroyed the impression of the light. Dilute sulphuric and acetic acids delay and weaken the action of the developer.

3. *Influence of Iodide of Potassium*—AgI paper moistened with a fresh solution of KI was insensible to light, and also to the developer

and paper moistened after exposure did not become colored by the developer; hence the author concludes that KI perfectly destroys the photographic sensibility of AgI both during and after the exposure.

The Effect of Fire on Cast Iron.

From the London Builder, No. 1075.

Attention has been directed by Dr. Percy, to the fact that several of the cast iron girders at the German Bazaar, were much bent, and the columns distorted, by the operation of the fire which occurred there some weeks ago. The subject is one of great importance with reference to the use of cast iron for building purposes. The girders in question were 18 feet long and 13 inches deep in the middle, tapering off slightly towards each end, and flanged, as usual, at the bottom. Of those entire after having fallen to the ground two were bent laterally in a striking and a nearly equal degree. The flexure was gradual from end to end. The deviation from a straight line at the end was 22 inches. No cracks could be anywhere detected. Many of the cast iron columns, which are still upright, Dr. Percy says, have been singularly twisted at the upper part, as though the metal there had been softened by heat, and had yielded without cracking, to the effect of pressure from above. As far as he could judge, there was no very decided evidence of fracture in either girders or columns from the injection of water upon them; and yet, from the fused glass and other objects which lay scattered about, it was certain that they must have been exposed to a pretty high temperature.

We may endorse Dr. Percy's observation, that a museum of objects in illustration of accidents, such as the bursting of boilers, breakage of railway axles and tires, railway collision, &c., would be as interesting as it would assuredly be important in a practical point of view.

New Process for Silvering Glass. By M. A. Martin.

From the London Artizan, Oct., 1863.

Among the large number of processes for silvering, Drayton's process is the best adapted for telescope glasses; but, as this process requires great skill on the part of the operator, I have endeavored to find some method, which, by its simplicity and sureness, might become general. After carefully studying and experimenting on all the known processes (aldehyde, sugar of milk, glucosate of lime, &c.), I have arrived at one, which, from its simplicity and the firm adherence of the layer of silver deposited, seems to fulfil all the necessary conditions. I begin by preparing:—1. A solution of 10 grammes of nitrate of silver in 100 grammes of distilled water. 2. An aqueous solution of pure ammonia, marking 13 degrees on Carter's areometer. 3. A solution of 20 grammes of pure caustic soda in 500 grammes of distilled water. 4. A solution of 25 grammes of ordinary white sugar in 200 grammes of distilled water. Into this pour 1 centimetre cube of nitric acid at 36 degrees, boil for twenty minutes, to produce the interversion of the sugar, and then make up the volume of 500 centimetres cube with distilled water, and 50 centimetres of alcohol at 36 degrees. This done, I prepare an argentiferous liquid by pouring into a flask 12 cubic centimetres of the solution of nitrate of silver (1), then 8 cubic centimetres of ammonia at 13 degrees (2), then 20 centimetres of the solution of soda (3); and lastly, make up a volume of 100 centimetres by 60 centimetres of distilled water. If the proportions have been properly observed the liquid will remain limpid, and a drop of solution of nitrate of silver will produce a permanent precipitate; then after being left quiet for twenty-four hours the solution is ready for use. Clean the surface to be silvered with a cotton plug impregnated with a few drops of nitric acid, then wash it with distilled water, drain and place it on supports on the surface of a bath, composed of the argentiferous liquid, to which has been added one-tenth or one-twelfth of the solution of sugar (4). Under the influence of diffused light the liquid becomes yellow, then brown, and after from two to five minutes the whole of the surface of the glass will be silvered; after ten or fifteen minutes it will have attained the required thickness; it must be washed first with ordinary water, then with distilled water, and stood upon its edge to dry in the air. The surface will then be covered with a light, whitish veil, easily removed by a little polishing rouge on chamois leather, leaving a brilliant surface perfectly adapted by its physical constitution for the purposes for which it is intended.

Railway Grease.

From the Chemical News, No. 207.

The process of manufacture is exceedingly simple. The fats are melted in a boiler, and brought up to 130° to 190° F. The water and soda crystals are heated in another vessel to 200° F., and both run into a wooden tub, with constant stirring, which is continued at intervals until the mixture is cold. Slow cooling insures a firm article, and large batches, which retain their heat for a considerable time, are therefore preferable. Care is taken for obvious reasons to avoid the introduction of sand or gritty matter. The proportions of materials vary in different establishments, and in the same establishments at different seasons of the year, the grease for July use being too firm for December, and *vice versa.* 25 per cent. is the lowest quantity of fat that can be safely used in the coldest weather, while 25 per cent. is amply sufficient for our warmest summer use. The following proportions have been used with excellent results:—

	Winter.			Summer.		
	cwt.	qr.	lb.	cwt.	qr.	lb.
Tallow . .	3	3	0	4	2	0
Palm oil . .	2	2	0	2	2	0
Sperm oil . .	0	1	7	0	0	27
Soda crystals .	1	0	14	1	0	8
Water . .	12	3	12	12	0	26

These proportions produced one ton of grease (allowing 2½ per cent.

for loss). That marked "summer" ran 1200 miles. The sperm oil, although small in quantity, has an excellent effect; but some makers, for economy, use rosin oil instead, although the result is questionable. —*Chemical Techology, by Richardson and Watts.*

Electro-Magnetic Phonograph.

From the Journal of the Society of Arts, No. 569.

This machine is capable of being attached to piano-fortes, organs, and other keyed musical instruments, by means of which they are rendered melographic, that is, capable of writing down any music that is played upon them.

So keenly have musicians at all times felt the extreme tediousness of writing music by hand, and the impossibility of preserving the most valuable impromptu pieces in their full and flowing beauty, that immediately on the introduction of the piano forte into England strenuous efforts were made by men of inventive skill to supply the instrument with the means of registering the music performed upon it. "The first piano-forte seen in England was made by one Father Wood, an English monk at Rome, and by him sold to Samuel Crisp, Esq., who sold it again to Fulke Greville, Esq., for one hundred guineas."* This was about 1757.

The Rev.—Creed would appear to have been one of the first, if not the first, to think of constructing a melographic instrument, and in the year 1747 he sent to the Royal Society a paper, entitled "A demonstration of the possibility of making a machine that will write extempore voluntaries, or other pieces of music, &c."†

There are also obscure accounts of a machine made in 1770, by a monk named Engramelle.

In a German work of 1774, John Fredrick Unger, a counsellor of justice at Berlin, claims priority of invention against Mr. Creed, though it seems most probable that each made a similar invention unknown to the other.

There is no doubt whatever that the Académie of Berlin was presented by Hohlfield—an ingenious mechanic who received some suggestions from Euler—with a machine which, to a limited extent, answered its purpose. It consisted of two cylinders moving paper between them, on which, by means of a crayon, each key made a mark when pressed down in the act of playing. But not only was the action of playing very fatiguing, but the music must have been of a most inconvenient width—that of the key-board—and without any stave, accidentals, &c.; in fact, a mere series of dots showing such and such keys were pressed down in the course of the performance, but utterly failing to mark the time, key, or accidentals. The Académie, however, in consideration of the great ingenuity of the contrivance, rewarded the inventor with a handsome gratuity.

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* Riambault's History of the "Piano-Forte."

† Phil. Trans. vol. xliv., p. 445.

In 1827, M. Carreyre made trial, before the Committee of the Fine Arts of the Institute of France, of a melographic piano, which consisted of a clockwork movement, which unwound from one cylinder to another a thin plate of lead, on which were impressed, by the action of the keys of the instrument, certain peculiar signs, which might be translated into the ordinary notation by means of an explanatory table.

"After the experiment, the plate of lead was removed, to make the translation, and a commission was appointed to report; but as no report was ever made, it is probable that the translation was not found to be exact. At the same time M. Baudouin read before the Institute a paper accompanying it with drawings, concerning another melographic piano; upon the merit of which we do not find that the Institute pronounced.*

These accounts prove two important facts; the great efforts made and the small success achieved—this want of success proceeded from the lack of a proper motive power, none having used electro-magnetism—for it must be evident to all acquainted with music that these were as yet nothing more than partially successful experiments, and produced no further result than stimulating inventors to continued exertions.

The causes of failure were many, the most serious being the oversight of endeavoring to derive the mechanical power from the keys of the piano, whereas some power, which, while depending upon the action of the key for its liberation and manifestation, should at the same time exert its force without strain upon the key, still remained a desideratum. Such a power is electro-magnetism, as the mere motion of a piano key, without any alteration in the touch required—may be made to call forth a force of any magnitude required. Now, in Unger's machine the power was derived from the keys alone, and by direct action, thus rendering the touch of the piano so heavy that no one could perform properly upon it. For this reason it is unnecessary to consider further its defects. M. Carreyre's, besides being equally objectionable on the score of its unavoidably heavy touch, and arbitary and unmeaning signs, produced at the best, but an indented sheet of lead, a medium for writing music on most inconvenient and unmanageable.

A machine which should register in plain black and white on common paper the music performed, giving the score on the ordinary stave, using the flat, sharp, and natural signs, as in all modern music, accurately registering time, bars, legato and staccato, 8va, alto, and basso passages, and adapted to all keys, still remained a desideratum, for from 1827 to 1863 no further progress was made, though many continued to give their attention to the subject.

But in 1863 Mr. Fenby applied electro-magnetism, and a machine was patented by him, January 13th, in that year, which, without altering the touch or appearance of an ordinary piano, is stated to be capable of registering the most complicated music.

Before giving any detailed description of the construction and capabilities of the phonograph, it may be well to point out the obstacles to be overcome in the notation, and thus to separate the possible from the impossible.

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* Riambault.

The most obvious difficulty—and one which if not overcome would render all other excellencies nearly, if not quite, futile—is the means of marking the various durations of the notes from the breve to the demisemiquaver, &c. This was a difficulty, inasmuch as the ordinary open, closed, and tailed notes cannot possibly be rendered available in an instrument registering that which is performed upon it.

The following considerations will render this apparent. The longest note is practically but the fusion of a number of shorter notes, from which it follows that on any particular key being depressed, as in playing, its first touch would be the shortest note of the notation, and the machine would immediately print such shortest note, and could not afterwards alter it; for to suppose a piece of machine to render shorter or longer notes by arbitrary signs, having but a fictitious relation to their duration,is to suppose its possession of a reasoning power, the absurdity of which needs no comment. From these considerations, and others which will readily occur to the mind of the reader, it is manifest that some system is required in which the duration of sound and the performance of the printing may be co-existent, and thus produce a complete reciprocity of action between the two. In other words, a short note must be capable of becoming a long one in the printing as in the playing.

Bearing these facts in mind will lead to a complete comprehension and appreciation of the system. Each note shows the portion of time occupied in playing it by the length it occupies in the bar, and consists of a horizontal black line proportionate in length to the duration of the note, while the rest of the notation needs no comment, it being in all respects identical with that at present in use.

Having considered the notation, the next thing to which our attention will naturally turn is the mechanical appliances employed to produce this notation. First, then, as to the touch of the piano: this remains, to all intents and purposes, the same as if without the phonograph attached, as the mechanical power is not derived from the motion of the keys, but from a voltaic battery; the only part performed by the key being to bring a small brass stud, on its under side, in contact with a slender spring; this causes an electro-magnet to bring a tracer against the paper which is continually moving at a fixed rate and thus marks the note. When the key is no longer depressed, the tracing ceases, and the rod slides back; this mechanism being capable of registering the slowest or most rapid playing. The accidentals are printed by revolving type, acted on by the same sliding-rod and magnet. The accidentals are adapted for all keys, so that any number of flats or sharps may be correctly registered; the machine being capable of distinguishing accidentals, flats, sharps, and naturals from those which are proper to the key in which the music may be pitched. That is to say, if the key of A natural be used, F, C, and G, when played sharp, will have no sharp sign in the body of the music, whereas if the naturals of these notes be struck, or the sharps of any others, suitable accidentals will be printed.

Having now reviewed the notes and signs proper to them, the bars will be considered. The barring of the music is performed in a simple manner, precluding the possibility of derangement, and is yet so accurate in adjustment that it correctly follows the accentuation of the most complicated piece of music. When a rallantando movement occurs, the bar or bars through which it runs will be actually lengthened in such a proportion as will accurately denote the character and expression of that part of the music. The same manifest advantages occur in the matter of legato and staccato movements.

The machine requires only blank paper, as it rules the stave and prints the score simultaneously. The inventor furnishes a small battery of convenient and simple form. The charge consists of sulphate of copper and water: one charge lasting for some months. The whole is in a neat drawer at the bottom of the machine, and offers nothing of difficulty or unpleasantness in its management, and requiring to be touched only to supply water to it.

Properties of a new Gun Metal.

From the Lond. Mechanics' Magazine, September, 1863.

Messrs. Deville and Caron have lately been making experiments on the properties of a new gun metal, a compound of silicium and copper. When copper contains rather less than five per cent. of silicium, it presents a fine bronze color, is fusible, and rather harder than bronze, but is perfectly ductile, and can be readily worked without clogging the tools as bronze does. Its tenacity is remarkable, being equal to that of iron. Silicium is the basis of sand, and the manufacture of its compounds with copper may be made by fusing together a mixture of sand, sodium, and copper, with some common salt and fluor spar as flux.

Soluble Silk.

From the Lond. Practical Mechanic's Journal, Oct., 1863.

This, which was the philosopher's stone on the search for which the late Mr. Schwabe of Manchester expended the energies of a life, has apparently at last been found by M. Persoz, the younger, and the *modus operandi* is due to the discovery of *dialysis* by our own Professor Graham, master of the mint. Persoz has found, as we noticed, many months ago, that silk is soluble in solution of chloride of lime, but he had sought in vain for any chemical agent by which the dissolved silk might be separated from its saline solvent. He at length tried dialysis, and by its means, separated the whole of the chloride of zinc, leaving the silk alone in the state of a limpid, colorless, tasteless liquid, which by evaporation or drying produces a gold-colored and *brittle* varnish. It remains still then to get back the silk with all its original physical properties unaltered.

The Production of Cast Steel directly from Pig Iron.

From the Lond. Mechanics' Magazine, Jan., 1864.

None of the foreign papers seem to have noticed the attempts of Cazanave to obtain cast steel directly from pig iron. The idea itself appears to be very ingenious, but of course the question is whether it is applicable in practice. The foundation of this new method is the influence of steam on a thin stream of pig iron. If we take an iron tube of a certain diameter with sides of the necessary strength, form a ring out of it, and fix on its circumference, towards the centre, three or more tubes, we have a tube ring with three or more radii. The radius is made fast to the tubular pipe; the ends of these tubes, which are open, do not quite reach to the centre of the ring, and have, therefore, between the ends an empty space, in which the pig iron is allowed to flow in a stream of a certain strength. The stream led from the boiler into the tubular pipe flows out of the openings of the three tubes, and operates directly upon the pig iron. It is said that the oxygen of the steam oxidizes the carbon of the pig iron, the silicium, a portion of the sulphur, phosphorus, and other impurities in the pig iron; the hydrogen combines with the carbon, sulphur, phosphorus, arsenic, and other bodies, with which it forms combinations of hydrogen. The carbonized and purified metal falls into a crucible or other vessel placed immediately under the apparatus. The metal obtained contains impurities, and must, therefore, be smelted in crucibles in a blast or reverberatory furnace. This is the essential part of the process; the simplicity of the method and the cheapness of the product are evident.

Now arises the questions:—Is it possible to obtain steel in large quantities by this method? will it be of the same quality as the small quantity obtained on trial? and, if it is possible, at what price can it be obtained?

In answer to these questions, Cazanave asserts that by his method steel can be obtained in great quantities, not inferior to the best steel, and proportionately cheaper; for his best quality steel can be obtained for £18 per ton. This is difficult to believe, but the inventor affirms that it is so, and at the same time warrants the excellent quality of his steel. In the present method of obtaining steel, good iron must be used, which is cemented, and the cemented iron, that is the steel, is smelted in crucibles. By Cazanave's method cementation of the iron is avoided, so that the cast steel may be obtained in unlimited qualities. If this new method turns out practicable, it will be possible to work up the whole daily production of a blast-furnace into steel. For this only the apparatus is required, which is not very costly, and which would be erected near the blast furnace and stream of pig iron. The stream would be divided into rays of the necessary strength, and each one directed into an apparatus. By Bessemer's process about ten tons of steel are obtained per day at Sheffield; while by Cazanave's method sixy and seventy tons per day could be obtained, and a blast furnace is being erected at Charleroi which will produce about seventy-four tons per day. The samples of steel furnished by this new process are reported to be very good. They were obtained from pig iron smelted with coke, but it is supposed that charcoal pig iron would give better results.—*Colliery Guardian.*

For the Journal of the Franklin Institute.

Valve Motion.—Cut-offs.

Mayer's, or as its modification is called in this country, Merrick's Valve.

By John D. Van Buren, Jr., C.E., 2d Asst. Eng. U.S.N.

The object sought in this paper is to obtain formulæ for designing the parts, and to discuss the circumstances of the motion, of this valve. This valve is a momentarily adjustable *Cut-off*. Fig. 1, shows an outline of a longitudinal section of the valve and its eccentrics. SS is the valve-seat; PP, are the ports leading to the cylinder AA, E its exhaust passage; *pp*, *steam* ports of the main valve VV, and *e* its exhaust-port. *ll* are two small slides driven by an independent eccentric over the back of the main valve, and adjustable upon the stem by means of right and left hand screws and nuts; so that by turning the stem the valves can be made to approach or recede from each other, according to the degree of *expansion* required. C is the shaft with eccentrics attached. To simplify the sketch the steam-chest, &c., are omitted. The action of the valve will be readily understood by reference to the figure. The steam can enter the cylinder only through the ports *pp*, and, by adjusting the valves *ll*, these ports can be closed at almost any point of the motion of the piston AA. It will be seen that the main valve is simply a modification of the ordinary three-ported locomotive slide. The valves *ll* are usually *roofed* over, and this roof or back connected with the condenser by means of a pipe and ring packing against the steam-chest bonnet, thus making the valve a *balanced* valve.

The following notation will be adopted in this discussion:

R=length of crank of shaft.

r_1= crank-arm or half throw of eccentric of main valve.

r=crank-arm or half throw of eccentric of cut-off valves *ll*.

c= cover or *lap* of main valve.

l= lead of main valve.

d=distance between outer edges of ports p and valves l when the main valve is in its *central* position.

v= length of faces of cut-off valves *ll*.

p= breadth of steam ports *pp*.

b=distance between *inner* edges of ports *pp*.

x=angle made by crank R with axis of cylinder, at the instant when steam is cut off.

θ=angle made by r_1 at same time, with same line.

Main Valve for Central Position. *Fig. 1.*

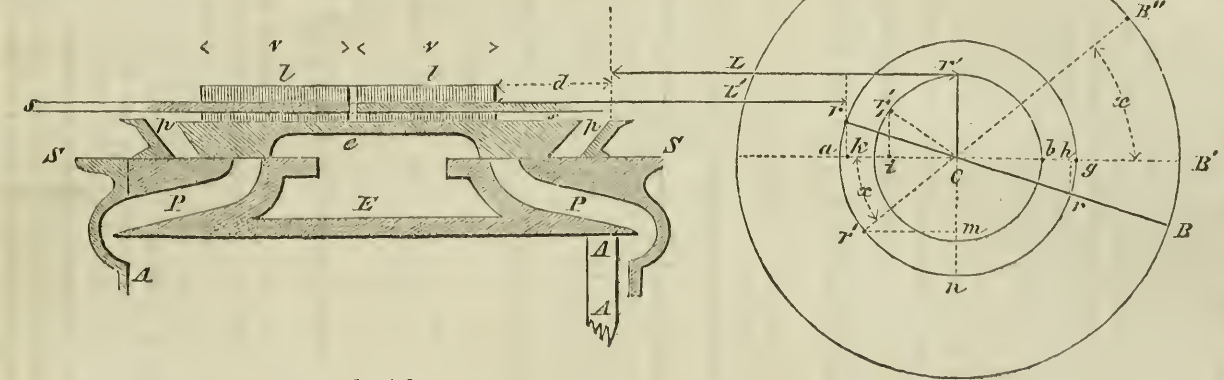

FIG. 1. Angle $rca = \sin.^{-1}\dfrac{c+l}{r_1}$

$kh = 2r\cos.\sin.^{-1}\dfrac{c+l}{r_1}$

Angle $bcr^1_1 = 90° + \sin.^{-1}\dfrac{c+l}{r_1} + x$

$ci = -r_1\cos.\theta$

$r^1m = r\cos.x$

$$\left.\begin{array}{l}\text{When edges of valves come together } L + ci = L' + mr' \\ \text{When main valve is in central position } L + d = L' + ck\end{array}\right\}\text{ Eq.(1)\&(1')}$$

N. B.—All angles measured round from CB'.

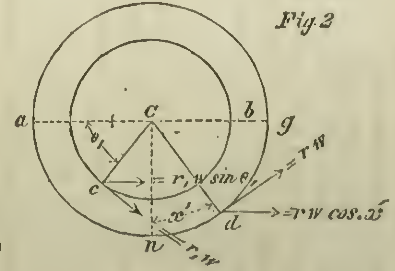
Fig. 2

Suppose now the crank is at B^1, or the piston at the top of its stroke. By following out the motion, it will be perceived, that to make the arrangement effective as an adjustable *cut-off*, the eccentric of the valves ll must be set 180° ahead of the crank R, provided no rock-shafts intervene between rr_1 and their respective valves. The eccentrics must be set 180° ahead of their proper position without rock-shafts, when rock-shafts are used. Let the eccentric rods be supposed to be always parallel to the axis of the cylinder. Then observing that the distances from the centre of the shaft to the edges of the port p and valve l are equal when the edges come together, we shall have for any angle x.

$$\mathrm{L}-r_1 \cos\theta=\mathrm{L}-r_1 \cos.(90^\circ+\sin\frac{^{-1}c+l^{*}}{r_1}+x)=\mathrm{L}'+r\cos.x \qquad (1)$$

But

$$\mathrm{L}+d=\mathrm{L}'+r\cos.\sin.\frac{^{-1}c+l^{*}}{r_1}. \quad . \quad . \quad . \quad . \qquad (1)'$$

and by substitution in (1)

$$d=r\cos.\sin.\frac{^{-1}c+l}{r_1}-r_1\cos.(90^\circ+\sin.\frac{^{-1}c+l}{r_1}+x)-r\cos x \quad (a)$$

$$\text{or,}=\frac{r}{r_1}\sqrt{r_1^2-(c+l)^2}-r_1\cos.(90^\circ+\sin.\frac{^{-1}c+l}{r_1}+x)-r\cos.x \qquad (2)$$

since $\sin.\sin.\frac{^{-1}c+l}{r^1}=\frac{c+l}{r_1}$, and $\cos.=\sqrt{1-\sin.^2}$

or, since $-\cos.(90^\circ+a)=+\sin a$.

$$d=\frac{r}{r_1}\sqrt{r_1^2-(c+l)^2}+r_1\sin.(\sin.\frac{^{-1}c+l^{\dagger}}{r_1}+x)-r\cos.x. \qquad (2)^1$$

The arc $\sin.\frac{^{-1}c+l}{r_1}$ being that through which r_1 must move from its central position to give $c+l$.

Formula $(2)'$ therefore shows how far the edge of l must be from that of p when the main valve is in its central position, in order to cause a cutting off of the steam when the angle of the crank is x.

Again, suppose the arms r_1 and r to have moved to the positions c and d, Fig. 2, respectively; both valves will now be traveling in the same direction, and if the velocities of the two valves are *equal* when the edges come together, then will the main valve get ahead of the cut-off valve, and the port p will *again* be opened if any further motion ensue (supposing, as we must, that l was *behind* p before the edges came together). Therefore, for that position of the crank, above *half-stroke*, which corresponds with *equal* velocities of the two valves, $l\,l$ are useless, and for all points beyond to the end of the stroke. The main valve must therefore be provided with sufficient *lap* to cut off the

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[*] Sin ^{-1}x means the arc or angle whose *sine is equal to x*, and similarly for cos^{-1}.

[†] L and L′ are the lengths of the eccentric-rods of r_1 and r respectively.

steam when the crank assumes this limiting position, and in practice the lap should be slightly greater than this amount.

To find this limiting position, refer to fig. 2, and let w be the angular velocity of the shaft, then by the conditions,

$$r_1\, w \sin.\theta_1 = r\, w \cos.\, x^1 \quad . \quad . \quad . \quad . \qquad (3)$$

$$\text{But } \theta_1 = x - \left(90^\circ - \sin.\frac{{}^{-1}c+l}{r_1}\right) = x + \sin.\frac{{}^{-1}c+l}{r_1} - 90^\circ$$

and $x^1 = x - 90^\circ$.

$$\therefore -r_1 \cos.\left(x + \sin.\frac{{}^{-1}c+l}{r_1}\right) = r \sin.\, x.$$

and since $\cos.\,(a+b) = \cos.\, a, \cos.\, b - \sin.\, a, \sin.\, b$, generally.

$$\therefore -r_1 \cos.\, x\, \frac{\sqrt{r_1^2 - (c+l)^2}}{r_1} + r_1 \sin x.\ \frac{c+l}{r_1} = r \sin x.$$

$$\therefore \tan.\, x = \frac{\sqrt{r_1^2 - (c+l)^2}}{(c+l) - r}. \quad . \quad . \quad . \quad . \qquad (4)$$

Therefore the main valve must have sufficient *lap* to cut off the steam when the crank makes an angle x, whose tangent $= \frac{\sqrt{r_1^2 - (c+l)^2}}{(c+l) - r}$ with the axis of cylinder. The positive sign must be used for the radical, and the tangent will then be *negative*, showing the angle to be over 90°.

To find how much is necessary to accomplish this end, we have (see *Bourne's Treatise, Slide-valve*),

$$c = r_1\sqrt{\frac{2\text{R} - \text{S}''}{2\text{R}}} - \tfrac{1}{2}\, l \quad . \quad . \quad . \quad . \qquad (5)$$

where S'' is the distance of the piston from the end of the stroke when the steam is cut-off.

$$\therefore \text{S}'' = \text{R}(1 - \cos.\, x).$$

$$\therefore c = \text{lap} = r_1\sqrt{\frac{1 + \cos.\, x}{2}} - \tfrac{1}{2}\, l \quad . \quad . \quad . \qquad (6)$$

and we must assume such a value to x in equation (6) that when c calculated by (6) is substituted in (4), the value of x found by (4) shall be *slightly greater* than the *assumed* value of x. This angle x is generally about 120°, and therefore one trial will generally fix how much lap (c) must be given in order that the *main valve* shall cut off before the cut-off valves begin to *uncover* the ports *pp*.

This maximum value of x substituted in equation (2) will give the maximum value of d required, and will therefore determine the length of the screw on the stem SS; and if $x = 0$, in equation (a), the value of d then obtained will be the proper distance between the edges of p and l, when the main valve is in its *central* position, necessary to cause the steam to be cut-off when the piston is at the top of its stroke. A value $x = -\sin.\,\frac{{}^{-1}c+l}{r_1}$, will give equation (a) $d=0$, as it should.

To prevent the *uncovering* of the *ports pp* at the *inner* edges, the faces of the valves ll must have a length corresponding with the maximum separation of the edges *after* steam has been cut off, *i. e.* l must equal the breadth of the port p plus the distance which the outer edge of l passes *over* the outer edge of p, when this distance is a maximum. Now suppose the steam cut off when $x=0$; it will be seen that the maximum value of d is the maximum distance to which the outer edge of l will attain beyond p and therefore,

$$\text{Breadth of face} = v = d + p \quad . \quad . \quad . \quad . \qquad (7)$$

where d has its maximum value.

There however must be a corresponding distance between the inner edges of the ports pp to accommodate all these conditions. Let a represent the distance between inner edges of valves ll, and suppose the main valve to be in its central position; then, we shall have

$$2v+a+d=2r\ \cos.\ \sin.\ \frac{{}^{-1}c+l}{r_1}+b+2p-d. \quad . \quad . \qquad (8)$$

Since for the *other end of the stroke* the outer edges of the other cut-off valve and port p, must be in the same relative position as the two under discussion.

$$\therefore b=2(v+d)-2\,r\cos.\ \sin.\ \frac{{}^{-1}c+l}{r_1}-2p+a. \quad . \qquad (9)$$

but we may make $a=0$, when the valves are adjusted for a maximum value of x; also $v=d+p$, and $r\cos.\sin.\ \frac{{}^{-1}c+l}{r_1} = \frac{r}{r_1}\sqrt{r_1^2-(c+l)^2}$

$$r \therefore b=4d-\frac{2r}{r_1}\sqrt{r_1^2-(c+l)^2} \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

where d has its maximum value, determined by the value of x in equation (4). Observe equation (8) that the valves ll travel $2r\ \cos.\ \sin.\ \frac{{}^{-1}c+l}{r_1}$, while the main valve passes from its central position around 180°.

Recapitulation.

$$d=\frac{r}{r_1}\sqrt{r_1^2-(c+l)^2}+r_1\ \sin.\left(\sin.\ \frac{{}^{-1}c+l}{r_1}+x\right)-r\cos.\ x. \qquad (11)$$

$$\left\{\begin{array}{ll} \tan.\ x=\sqrt{\dfrac{r_1^2-(c+l)^2}{(c+l)-r}}. \quad . \quad . \quad . & (12) \\ c=r_1\sqrt{\dfrac{1+\cos.\ x}{2}}-\tfrac{1}{2}l. \quad . \quad . \quad . \quad . & (13) \end{array}\right.$$

$$v=d+p \quad . \quad . \quad . \quad . \quad . \quad . \qquad (14)$$

$$b=4d-2\,\frac{r}{r_1}\sqrt{r_1^2-(c+l)^2}. \quad . \quad . \quad . \quad . \qquad (15)$$

The main valve is designed in the same manner as the common locomotive slide. It will be observed that r_1 must *at least* be equal to $dc+p$, in order that the port may be fully opened; and therefore $r_1=c+p$ determines r_1. The *lead* is assumed, according to the judgment of the designer, but generally varies from $\frac{1}{16}$ inch to $\frac{3}{8}$ inch, for engines with high velocities the *lead* being greater than is required for engines with low velocities. This lead will of course depend somewhat upon the *length* of the port p and the capacity of the cylinder, but may safely be assumed between the above limits. If $r_1=r$ the formulæ will be somewhat simplified.

Example.

Let $r_1=5''$, $r=5\cdot5''$, $l=0\cdot2''$.

Now assume that the cut-off valves are efficient only up to a value of $x=120°$, or for $\frac{3}{4}$ths the stroke of piston ; therefore equation (13).

$$c=5\sqrt{\frac{1+\cos.\ 120°}{2}}-\tfrac{1}{2}l=5\sqrt{\frac{1-\frac{1}{2}}{2}}-0\cdot1''=\tfrac{5}{2}''-0\cdot1=2\cdot4''$$

substituting this value of c in equation (12),

$$\therefore \tan.\ x=\frac{\sqrt{25-2\cdot6^2}}{2\cdot6-5\cdot5}=-\frac{\sqrt{18\cdot24}}{2\cdot9}=-1\cdot4727=\tan.\ 124°\ 11'$$

or by logarithms

$$\log.\ \tan.\ x=\log.-\sqrt{\frac{18\cdot24}{2\ 9}}=-0\cdot1681144=\log.\ \tan.\ 124°\ 11''$$

Therefore this value of c is sufficient, and the main valves will cut off the steam before the limiting angle for ll is attained—being 4°11′ before the edges of the valve l comes in contact with p, the steam-port of vv; d need not therefore be calculated for an angle greater than 120°.

$$\therefore \text{(eq. 11)}\ d=\frac{5\cdot5}{5}\times\sqrt{25-2\cdot6^2}.+5\sin.\left(120°+\sin.\frac{-'2\cdot6}{5}\right)+$$
$$5\cdot5\times\tfrac{1}{2}$$

$$d=1\cdot1\sqrt{18\cdot24}+5\sin.\ (120°+\sin.\ ^{-1}\cdot52)+2\cdot75.$$

But the arc whose sine is $0\cdot52=31°\ 20'$

$$\therefore d=1\cdot1\sqrt{18\cdot24}+5\sin.\ (120°+31°\ 20')+2\cdot75$$
$$=4\cdot697+5\sin.\ 28°\ 40'+2\cdot75$$

since $\sin.\ (180-x)=\sin x$.

$$\therefore d=4\cdot697+5\times0\cdot47971+2\cdot75=9\cdot85''$$

But $v=p+d=r_1-c+d=5-2\cdot4+9\cdot85=12\cdot45''$

and $b=4d-\frac{2r}{r_1}\sqrt{r_1^2-(c+l)^2}=39\cdot40-9''\cdot394=30''\cdot016.$

The length of the screws on the stem ss must therefore be $d+$length of the nut.

Much of the arithmetical work above, might have been omitted, but it is thought the method will be more readily comprehended by giving *all* the details of the computation.

The small valves are therefore efficient between the limits $x=0$ and $x=120°$, and the engine may be stopped by means of the expansion gear. By contracting these limits the ports may be made smaller. For instance, if it is not required to cut off the steam below $\frac{1}{4}$th the stroke from commencement, *i. e.* to expand the steam more than through three-fourths the stroke, the value of *v may be diminished by the value d has for* $x=60°$· *b* will be diminished by *twice* the value *d* has for $x=60°$.

The above dimensions are by no means meant for a criterion, since the *assumptions* must vary with the case and judgment of the designer; but are simply used as an illustration—although correct for the assumed conditions.

Naval Academy, Newport, R. I., Jan. 25, 1864.

An Account of some Researches on Radiant Heat. By JOHN TYNDALL, ESQ., F.R.S., Prof. Nat. Phil., Royal Inst.

In his former researches on the radiation and absorption of heat by gaseous matter, the speaker compared different gases and vapors at a common thickness with each other; one part of his present object was to compare different thicknesses of the same gaseous body with each other as to their action upon radiant heat. A few years ago he would be deemed a bold man who would attempt to measure the action of an inch, or indeed of many feet of a gas, on radiant heat; but the present experiments commence with plates of gas only 0·01 of an inch in thickness, and extend to thicknesses of 49·4 inches. Thus, the greatest thickness is to the least nearly in the ratio of 1 to 5000. The apparatus employed for the smaller thicknesses was a hollow cylinder, one end of which was closed by a plate of rock-salt. Into this fitted a second cylinder, with its end also closed by a plate of the salt. One cylinder moved within the other like a piston, and by this means the two plates of salt could be brought into flat contact with each other, or could be separated to any required distance. The distance between the plates was measured by a *vernier.* The cylinder was placed horizontal, being suitably connected with a source of heat. This latter consisted of a plate of copper, against which a steady sheet of flame was caused to play.

The absorption of radiant heat by carbonic oxide, carbonic acid, nitrous oxide, and olefiant gas was determined with this apparatus, and such differences as might be anticipated from former researches were found. Olefiant gas maintained its great superiority over the other gases at all thicknesses. A layer of this gas, not more than 0·01 of an inch in thickness, intercepted about 1 per cent. of the total radiation; and the delicacy of the apparatus may be inferred from the fact that this absorption—great, relative to the thickness of the layer of gas, but small absolutely—corresponded to a deflection of 11 degrees of the galvanometer. (It would be certainly possible to measure the action of a layer of this gas of less thickness than the paper on which these words are printed.) A layer of olefiant gas, 2 inches in thickness, intercepts nearly 30 per cent. of the entire radiation. The influence of a diathermic envelope surrounding a planet may be strikingly illustrated by reference to this gas. A shell of olefiant gas, 2 inches thick, surrounding the earth, would offer no appreciable hindrance to the solar rays in their earthward course; but it would intercept, and in great part return, 30 per cent. of the terrestrial radiation; under such a canopy the surface of the earth would probably be raised to a stifling temperature. A layer of the gas, $\frac{3}{10}$ of an inch thick, intercepts 11·5 per cent. of the whole radiation. Such a layer, if diffused through a stratum of air 10 feet thick, would be far more attenuated than the aqueous vapor actually diffused through the air; still it would produce an absorption greater than that which the speaker had assigned to the atmospheric vapor within 10 feet of the earth's surface. In the presence of such facts, the arguments which we might be disposed to base on the smallness of the quantity of atmospheric vapor are entirely devoid of weight.

In measuring the action of larger thicknesses of gas, the following method was pursued:—A brass cylinder, 49·4 inches in length, had its two ends stopped with plates of rock-salt, and a suitable source of heat placed at one end; the rays from this source passed through the tube, and were received by a thermo-electric pile placed at its opposite end; this radiation was exactly neutralized by the heat emitted from a cube of boiling water and incident on the opposite face of the pile. The interception of any portion of the heat emanating from the source by a gas or vapor introduced into the tube destroyed the equilibrium previously existing, and the amount intercepted was declared by the galvanometer. The thickness traversed by the calorific rays was varied in the following way:—The tube was divided into two distinct compartments by the introduction of a third plate of rock-salt. Let us agree to call the compartment most distant from the pile the *first chamber*, and that adjacent to the pile the *second chamber*. The experiments began with the first chamber short and the second chamber long, and ended with the first chamber long and the second chamber short. The alteration consisted solely in the shifting of the intermediate plate of salt, which lengthened the first chamber and diminished the second one by the same quantity; the sum of the lengths of both chambers being the constant quantity, 49·4 inches.

The absorption effected in the first chamber acting alone was first determined; then the absorption effected in the second chamber acting alone; and, finally, the absorption effected when both the chambers were occupied by the gas or vapor. This arrangement enabled the speaker to check his experiments, and also to examine the effect of the *shifting* which occurred in the first chamber on the absorption of the second one. The thermal coloration of the various gases was rendered strikingly manifest by these experiments. For the vast majority of the rays, for example, carbonic oxide and carbonic acid are transparent. Placing a stratum of carbonic oxide, 8 inches in length, in front of a column of the same gas, 41·4 inches long, these 8 inches intercepted 6 per cent. of the whole radiation; placed *behind* a column 41·4 inches long, the absorption of the same 8 inches was sensibly *nil.* So also with carbonic acid; 8 inches in front absorbed $6\frac{1}{4}$ per cent., while placed behind the effect was almost zero. Similar remarks apply to the other gases, the reason manifestly being that when the 8-inch stratum is in front, it stops the main portion of the rays which give it its thermal color, while, when it is placed behind, these same rays have been almost wholly withdrawn, and to the remaining 94 per cent., or thereabouts, of the radiation the gases are sensibly transparent.

An extension of this reasoning enables us at once to conclude, that the sum of the absorption of the two chambers taken separately must always be greater than the absorption effected by a single column of the gas of a length equal to the sum of the two chambers. This conclusion is illustrated in a striking manner by the experiments; and it is further found that when the mean of the sums of the absorption is divided by the absorption of the sum, the quotient is sensibly the same for all gases. It may also be inferred from considerations similar to the foregoing, that the sum of the absorptions must diminish, and approximate to the absorption of the sum, as the two chambers become more unequal in length, and that the sum of the absorptions of the two chambers is a maximum, when the medial rock-salt plate divides the long tube into two equal compartments.

In these days a special interest attaches itself to the radiation of any gas through itself or through any other gas having the same period of vibration. The speaker referred to the results of an elaborate series of experiments on this interesting question. The experimental tube, 49·4 inches long, was divided into two compartments by a partition of rock-salt. All external sources of heat were abolished, and the pile, furnished with its conical reflector, stood at the end of the tube. The compartment nearest the pile contained the gas which was to act as absorber, while that most distant from the pile held the gas which was to act as radiator. It is known that the destruction of the motion of a sensible mass of matter is always accompanied by the evolution of heat. A weight falling to the earth, and a ball striking a target, are heated on collision. The same is true for atoms, and in the present experiment the gas in the radiating chamber was heated by the collision of its own particles against the inner surface of the tube when they rushed in to fill the vacuum. The radiation was, in fact, what the speaker had named "dynamic radiation." The lengths of the two chambers were varied, the radiating column being lengthened and the absorbing one shortened at one and the same time; the sum of both was always the constant length 49·4 inches.

The experiments with the vapors were thus executed. Both the chambers into which the tube was divided were, in the first place, occupied by the vapor to be examined; the usual pressure being $\frac{1}{60}$ of an atmosphere. The entrance of the vapor was so slow, and its quantity so small, that the radiation due to the warming of the vapor by its own collision was insensible. The needle being at zero, dry air was allowed to enter the chamber most distant from the pile. This air became heated dynamically, communicated its heat to the vapor, and the latter immediately discharged the heat thus communicated to it against the pile. It is quite evident, that not only does this case resemble, but that it is actually of the same mechanical character as that in which a vibrating tuning-fork is brought into contact with a surface of some extent. The fork, which before was inaudible, becomes at once a copious source of sound. What the sounding-board is to the fork, the compound molecule is to the elementary atom. The tuning-fork vibrating alone is in the condition of the atom radiating alone, the sound of the one and the heat of the other being alike insensible. But in association with sulphuric or acetic ether-vapor the elementary atom is in the condition of the tuning-fork applied to its sound-board, communicating through the molecule motion to the luminiferous ether, as the fork through the board communicates its motion to the air.

The experiments demonstrate the great opacity of a gas to radiations from the same gas. They also show in a very striking manner the influence of attenuation in the case of vapor. The individual molecules of a vapor may be powerful absorbers and radiators, but in thin strata they constitute an open sieve through which a large quantity of radiant heat may pass. In such thin strata, therefore, the vapors, as used in our experiments, were generally found far less energetic than the gases, while in thick strata the same vapors showed an energy greatly superior to the same gases. The gases, it will be remembered, were always employed at a pressure of one atmosphere.

A few striking experiments were referred to in illustration of the influence of a paper lining, or a coat of varnish or lampblack, within the experimental tube. In dynamic radiation it is not possible to do entirely away with the action of the interior surface of the tube itself. When the tube is of brass and well polished within, the entrance of the air produces a deflection of 7·5 degs., this being due to the emission from the warmed surface of the tube. A lining of paper two feet long raises the radiation sufficiently to drive the needle through an arc of 80 degrees, while a ring of paper 1½ inches long placed within the tube radiates sufficient to urge the needle through an arc of 56 degrees.

The speaker finally examined the diathermancy of the liquids from which his vapors were derived, and the result leaves no shadow of a doubt upon the mind, that both absorption and radiation are molecular phenomena, irrespective of the state of aggregation. If any vapor is a strong absorber and radiator, the liquid whence it comes is also a strong absorber and radiator. The molecule carries its power, or want of power, through all its states of aggregation. The order of absorption in liquids and vapors is precisely the same; and the speaker looked forward with hope to the application of these results to other portions of the domain of thermotics.

Proceedings Royal Institution, June 12, 1863.

Boiler Explosion on the Great Western Railway.

From the London Practical Mechanic's Journal, Feb., 1864.

The inquiry into the cause of the recent explosion on the Great Western Railway took place, 20th November, 1863, near the scene of the occurrence. It will be remembered that Dr. Lankester adjourned the inquests on the deaths of the three men who were killed by the bursting of the engine, in order that he might have scientific evidence in respect of the state of the boiler both before and after the explosion, and also on the subject of the management of locomotives generally. Captain Tyler, R. E., Government Inspector of Railways, attended, and assisted in examining the other witnesses. The officer also submitted himself for examination, and read a detailed report which he had drawn up after an inspection of the remains of the Perseus. John Thompson, the driver of the Perseus, stated that on the morning of the day before that on which the accident took place he drove the engine from Swindon to London. In the course of that journey he drove it with a pressure of 115 lbs. on the square inch, and he observed nothing wrong with its boiler. He made a note in the engine-drivers' book of some slight repairs that had to be done with other parts of the Perseus, and the boiler was washed out. The Perseus had been running 12 years, but there were hundreds of older engines on the Great Western line. Mr. L. Young, boiler maker in the company's workshop at Swindon, stated that the last occasion on which the interior of the boiler of the Perseus was thoroughly examined was in 1855, when new pipes were put into it. Since then repairs had been done to the boiler on one or two occasions; but he had seen nothing to make him think that there was corrosion in the place where it subsequently gave way. When there was a suspicion of weakness in any part of a boiler, small holes were tapped and a gauge was introduced to try the thickness of the metal; but if there was no such suspicion no regular examination of the boiler took place except when new pipes were being put in. The pipes put into the Perseus in 1855 were in it when it exploded, and in the interim the engine had run 175,000 miles. The duration of pipes was usually measured by the number of miles which the engine had run, and they usually lasted for a mileage of from 100,000 to 200,000 miles. Mr. Daniel Gooch, chief engineer of the locomotive department, was the next witness. He stated that, including both broad and narrow gauges, there were about 500 engines in the rolling stock of the Great Western Company. The Perseus was a first-class broad-gauged engine, and was not an old one as compared with others belonging to the company. It was 12 years old, while there were more than 200 which were much older. Some had been running 24 or 25 years without new boilers. The Perseus had never had new boilers. It was the duty of each engine-driver after every journey to make a written report of anything that his locomotive required to have done to it, and the engine was not let out again until the necessary repairs were completed. The Perseus had 303 pipes. If the pipes of an engine were taken out they could not be put in again. New ones must be supplied, and they cost from 25s. to 27s. each. In reply to Captain Tyler, Mr. Gooch said that he did not approve testing boilers for strength of plates by the hydraulic process. This test was applied when the engine was at rest, and the strain to which the boiler was subjected, and which was much greater than any that it was ever required to bear when running, was calculated to injure it. He was persuaded that if the hydraulic test was applied they would have many more explosions. None of the great companies applied it. The engineers of the London and North-Western, the Great Northern, and the South-Western were against it. Captain Tyler observed that the hydraulic test was applied by the engineers of some lines, and with very beneficial results. Some locomotives had given away under it. Mr. Gooch further stated that he had several boilers of greater age than that of the Perseus tested, and he found them all sound. As to pipes, there was one engine on the line which had run 280,716 miles with the same set of pipes. Mr. Gooch, as well as the engine-driver and boiler maker, attributed the bursting of the boiler to corrosion of a part of one of the plates in the centre of the boiler barrel. Captain Tyler concurred with the officers of the company as to the immediate cause of the accident, but thought that the hydraulic test might have been applied with great advantage, and expressed his opinion that the running of an engine for seven years without a thorough examination of the interior of the boiler was a most dangerous proceeding. He observed, however, that there was no Government regulation on this point, and that there was considerable difference of opinion as to the merits of the hydraulic test. He brought under the notice of the Court and the officials of the Great Western Company a boiler manufactured by a firm at Ipswich, in which the pipes were so placed that they could be removed in a mass and replaced again. This invention had not as yet been applied to railway locomotives, but was confined to agricultural engines. It was not, however, clear to him that it might not be made available for railway engines. The jury found that the death of John Elridge, James Wilson, and Christopher Thomson (the men killed by the explosion), was accidental; but they recommended to railway companies a periodical examination of the boilers of their locomotives at intervals not exceeding three years.

Here we have exactly the case of the Rugby explosion over again, and the same dogmatic absurdity on the part of the locomotive engineering evidence—absurdity, if not something worse, that scarcely needs dissection. Ah! these tubes, that cost 27s. a piece, and would so probably blow out often in the hydraulic test—*hinc illæ lacrymæ.* What a comfortable assurance to Great Western travelers is that of Mr. Gooch! The engine that exploded was 12 years in use, and *had never been thoroughly examined for eight years;* but that is nothing, the Company have 200 engines a great deal older, and in worse condition!—Ed.

Tides of the White Sea.

From the London Athenæum, Jan., 1864.

The question as to the existence of a diurnal periodicity in the tides of the White Sea may now be considered as decided in the affirmative; for we find in the last number of the *Bulletin* of the Imperial Academy of Sciences of St. Petersburg a short notice of a report presented by Messrs. Lenz and Kupffer, in which the fact is stated for the first time. M. Zaroubine, an officer of the Corps of Pilots at Archangel, has made a long series of observations, and drawn up tables of the results, which have been laid before the Academy, and discussed by the reporters, who affirm his conclusion that a diurnal periodicity in the flux and reflux of the tides in the White Sea is thereby demonstrated. The report and a selection of the tables are to be published; but, we fear, in Russian only, and in a Russian periodical.

Talmi Gold.

From the London Chemical News, No. 213.

A beautiful gold-colored alloy, sold under the above name, gave, on analysis, the following results :—

Copper	86·4
Zinc	12·2
Tin	1·1
Iron	0·3

The iron was probably an accidental ingredient. The alloy besides was very thinly gilt. It is a good deal used to make watch-chains.

Centralblatt, No. 52, 1863.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, February 18, 1864.

William Sellers, President, in the chair.

John H. Towne, Vice President.

Robert Briggs, Corresponding Secretary.

Washington Jones, Recording Secretary.

The minutes of the last meeting were read and approved.

A letter was read from the Smithsonian Institution, Washington, D. C.

Donations to the Library were received from the Royal Astronomical Society, the Royal Institution, the Statistical Society, and the Commissioners of Patents, London; la Société Industrielle de Mulhouse, France; Prof. A. Dallas Bache, the Smithsonian Institution, and Thos. U. Walter, Esq., Washington, D. C.; Hon. Mr. Brady, Senate, Harrisburg, Pa.; the Pennsylvania Institution for the Blind, Prof. John F. Frazer, and John S. Cassin, Esq., Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's report for January was read.

The Board of Managers and Standing Committees reported their minutes.

The Board of Managers reported the appointment of the following Standing Committees of the Board.

On Publications.

John H. Towne,
Fairman Rogers,
Robert Briggs,
B. Henry Bartol,
George Harding.

On Instruction.

John F. Frazer,
Pliny E. Chase,
J. Vaughan Merrick,
Coleman Sellers,
Samuel Sloan.

Managers of Sinking Fund and Finance.

George Erety,
Washington Jones,
John F. Frazer,
Percival Roberts,
Samuel White.

Candidates for membership in the Institute (6) were proposed, and the candidates proposed at the last meeting (15) were duly elected.

On motion, a committee of five were appointed to revise the By-Laws, and report, if in their judgment they needed amendments.

Committee, Messrs. J. Vaughan Merrick, Washington Jones, Bloomfield H. Moore, Pliny E. Chase, John H. Towne.

On motion, a committee of twelve were appointed to consider the weights and measures, and coinage of the United States, with regard to the proposed introduction of the decimal system.

Committee, Messrs. Robert Briggs, Fairman Rogers, J. Vaughan Merrick, John F. Frazer, John H. Towne, B. Henry Bartol, John W. Nystrom, Washington Jones, Coleman Sellers, James C. Booth, William B. Bement, Samuel J. Reeves.

The Standing Committees for the ensuing year were appointed by the President, and approved as follows:—

On the Library.

Henry Ames,
Pliny E. Chase,
James H. Cresson,
James Dougherty,
John Ferguson,
George Harding,
Raper Hoskins.
James T. Lukens,
Henry G. Morris,
William H. Merrick.

On Cabinet of Minerals and Geological Specimens.

Robert Briggs,
Isaac H. Conrad,
John F. Frazer,
J. Peter Lesley,
Samuel J. Reeves,
Robert E. Rogers,
B. Howard Rand,
Coleman Sellers,
Richard Tilghman,
Elias Wildman.

On Cabinet of Models.

James Agnew,
William B. Bement,
Charles H. Cramp,
Mordecai W. Haines,
John Kile,
W. Barnet Le Van,
Charles T. Parry,
John L. Perkins,
Samuel Sloan,
S. Lloyd Wiegand.

On Cabinet of Arts and Manufactures.

William Adamson,
John H. Cooper,
Charles G. Crane,
William Divine,
Henry R. Lawrence,
Percival Roberts,
Newbold H. Trotter,
Eliashib Tracy,
Samuel S. White,
William Weightman.

On Exhibitions.

Robert Briggs,
James H. Cresson,
John Gardiner, Jr.,
Edwin Greble,
Bloomfield H. Moore,
J. Vaughan Merrick,
William A. Mitchell,
T. Morris Perot,
John H. Towne,
O. Howard Wilson.

On Meetings.

B. Henry Bartol,
Charles S. Close,
Henry Cartwright,
George Erety,
Henry Howson,
Edward Longstreth,
John W. Nystrom,
Percival Roberts,
Thomas Shaw,
Montgom'y P. Simons.

Charles M. Cresson,
John F. Frazer,
Henry Hartshorne,
James Hunter,
James A. Kirkpatrick,
Henry Morton,
James A. Meigs,
Fairman Rogers,
James S. Whitney,
Samuel S. White.

Mr. Chase continued the abstract of his papers on Tides of Rotation.

The principal theories that have been brought forward to account for the barometric tides, attribute them to

1. Variations of temperature,
2. Variations of moisture,
3. Formation and dissipation of clouds,
4. Electrical action of the sun,
5. Gravitation,
6. Centrifugal force,
7. "Rotation of the earth, and its connexion with the solar system."

[*W. C. Redfield*, in Silliman's Journal, vol. xxv, page 129.]

No one has attempted to point out any minute or precise correspondence between theory and observation, nor to furnish any satisfactory demonstration of the connexion between the observed phenomena and their supposed causes.

The prevailing sentiment of the day appears to incline towards the temperature theory, notwithstanding the confessedly inexplicable difficulties that attend it. James Hudson, (London Phil. Trans., 1832) points out "the general relation between the barometrical changes and the variations of temperature," but he admits that the relation "appears to be DIRECT during the morning hours, and INVERSE during those of the day and evening" Sir John Herschel says that "heat causes diurnal variations, but the effects surpass the natural operation of those causes." Prof. Espy, (4th Meteorolog. Rep. p. 12,) attempts to reconcile the American observations with his view of the heat theory: his explanations, though plausible, are unsatisfactory and will not bear the test of rigid scrutiny.

Each of the other enumerated causes undoubtedly exerts an influence which must be carefully investigated, before we can obtain a thorough knowledge of the laws which control the atmosphere. Such an investigation will probably show a mutual connexion through which all the secondary causes may be referred to a single force. Mr. Redfield's hypothesis, which is expressed in terms sufficiently indefinite and general to include all the rest, was anticipated by Galileo, who attributed the ocean tides "to the rotation of the earth, combined with its revolution about the sun." It appears that Galileo's opinion attracted little attention, and led to no special investigation, partly, perhaps, because it could hardly be reconciled with the tidal intervals, and partly because a literal, as well as figurative, reasoning in a circle, apparently demonstrated that the motions in question could produce no disturbing force. I have endeavored to show the fallacy of this notion, and to justify my reasoning by exhibiting the almost precise coincidence between the results of my hypothesis, and those of observation. My views are still further confirmed by the fact that the law of tidal variation derived from an exclusive reference of the aerial motions to a supposed stationary earth, is precisely the same as the law that is derived from the consideration of the relative attractions of two bodies revolving about their common centre of gravity.

The following table furnishes material for many instructive comparisons, some of which deserve special notice on account of the additional confirmations that they furnish to the rotation theory. The column headed "observed height of barometer," gives the grand mean of three years observations at St. Helena; A represents the theoretical height as computed by our formula; B introduces such modifications as would result from assuming the mean of the equatorial observations as a normal equatorial altitude.

TABLE OF MEAN BAROMETRICAL RESULTS, THEORETICAL AND OBSERVED.

Time.	Ratio of Observed Heights.	Observed Height of Bar. 28 in+	Theoret. Height A 28 in.+	Error A.	Theoret. Height B 28 in.	Error. B	Diff. of Errors.
0 h	1·00060	·2985	·2970	—15	·3002	17	—32
1	1·00000	·2819	·2821	2	·2821	2	0
2	·99945	·2660	·2672	12	·2640	—20	32
3	·99905	·2553	·2563	10	·2507	—46	56
4	·99894	·2521	·2523	2	·2458	—63	65
5	·99908	·2562	·2563	1	·2507	—55	56
6	·99938	·2642	·2672	30	·2640	— 2	32
7	·99982	·2764	·2821	57	·2821	57	0
8	1·00028	.2899	·2970	71	·3002	103	—32
9	1·00065	·3003	·3079	76	·3135	132	—56
10	1 00086	·3061	·3119	58	·3184	123	—65
11	1·00074	·3025	·3079	54	·3135	110	—56
12	1·00033	·2913	2970	57	·3002	89	—32
13	·99988	·2777	·2821	44	·2821	44	0
14	·99938	·2646	·2672	26	·2640	— 6	32
15	·99908	·2562	·2563	1	·2507	—55	56
16	·99905	·2550	·2523	—27	·2458	—92	65
17	·99926	·2611	·2563	—48	·2507	—104	56
18	·99973	·2737	·2672	—65	·2640	—97	32
19	1·00028	·2898	·2821	—77	·2821	—77	0
20	1·00081	·3048	·2970	—78	·3002	—46	—32
21	1·00120	·3163	·3079	—84	·3135	—28	—56
22	1·00130	·3184	·3119	—65	·3184	0	—65
23	1·00107	·3117	·3079	—38	·3135	18	—56

It will be seen that the purely theoretical height A corresponds more nearly with the observations than the mixed height B. It is therefore evident that there is a slight disturbance, (which may perhaps be owing either to variations of temperature, or to a resisting medium,) which follows a different law from the principal disturbance.

The changes are least near the times of high and low tide, and greatest midway between the two tides. If we compare the average high and low tides, we see that the observed height is somewhat less at high tide, and somewhat greater at low tide, than theory would give. These results would naturally follow from the combined fluidity and gravitation of the air.

From 1h. to 15h. inclusive (during most of which time the radius vector of each particle of air is increasing,) the observed height of the barometer is less than the theoretical height.

From 16h. to 0h. inclusive, (radius vector diminishing,) the observed height is greater than the theoretical height.

The greater pressure before noon than before midnight, is precisely the result which would follow from the passage of the earth through a resisting medium, but it is directly opposed to the supposed tendencies of varying temperature.

The apparent difference in the laws that govern the aerial and ocean tides may be partially, if not wholly, accounted for by considering the difference of constitution in the two media, and the relative positions of the observer. The air is highly elastic and compressible, while water is cohesive and incompressible; the observer is placed underneath the atmosphere, but above the ocean. The air can therefore readily yield to any expanding or condensing force, without much perceptible motion, while a similar force applied to water would produce motion in the direction of least resistance; any force that tends to throw fluids away from any given portion of the earth, produces a high aerial tide, but a low barometric tide, and after some interval a high oceanic tide.

The frequent coincidence of high water with a low barometer, has been noticed by many observers, and it is strikingly presented in the comparative drawings given by Lubbock in his Theory of the Tides. The prompt effect of rotation, combined with the retardation of the cumulative action which produces the lunar tides, may perhaps account for the errors of theory in Lubbock's Table of the

SEMI-MENSTRUAL INEQUALITY AT LONDON.

Apparent Solar Time of Moon's Transit B.		Height.		
		Theory.	Observation.	Error.
h.	m.	feet.	feet.	feet.
0	30	22·72	22·72	0
1	30	22·54	22·44	+·10
2	30	22·07	21·92	+·15
3	30	21·36	21·14	+·22
4	30	20·55	20·23	+.32
5	30	19·83	19·57	+·26
6	30	19·56	19·55	+·01
7	30	19·93	20·26	—·33
8	30	20·69	21·15	—·46
9	30	21·49	21·89	—·40
10	30	22·16	22·42	—·26
11	30	22·59	22·70	—·11

The regular recurrence of the aerial tides at stated hours, is a sufficient evidence of their dependence upon the relative positions of the earth and sun. Though the differential effect of the moon's attraction is greater than that of the sun's, the intensity of the solar attraction is much the greater. I am inclined to believe that this intensity is manifested in a greater stability of the solar attraction-spheroid, which prevents its yielding readily to the effects of rotation.

Lubbock quotes from Williams' Narrative of Missionary Enterprises, p. 172, his remarks on the "well-known fact that the tides in Tahiti and the Society Islands are uniform throughout the year, both as to the time of the ebb and flow, and the height of the rise and fall; it being high water invariably at noon and midnight, and low water at six in the morning and evening. The total range from low to high water seldom exceeds eighteen inches or two feet." The earth's rotation, producing an alternate half-day's acceleration and retardation in the eastward motion of the water, should create a tendency to tides of this character, and the situation of the islands mentioned is peculiarly favorable for the development of that tendency. Were they near a continent or at the entrance of a gradually narrowing ocean, they would feel the influence of the derivative tide which accumulates the attractive energies of the moon for several successive transits, and the tides would vary with the moon, as upon our own shores: but the nearly uninterrupted ocean sweep of 80° to the eastward may give the combined rotation and solar waves such resistless force, that they can easily overcome the weak intensity of the lunar attraction. If this hypothesis is confirmed by more accurate observations, the theory of Galileo will not only help us in our explanations of the aerial tides but it will also lead to the recognition of a most important element in the ocean tides.

Messrs. Cornelius & Baker exhibited a very beautiful method of *lighting gas* by means of frictional *electricity*, arranged for use with a bracket, two portable lighters, and a table light, all being simple in arrangement and readily kept in order.

These instruments are constructed upon the principle of the electrophorus.

The electric *bracket* is arranged with a brass cup in the form of a vase, resting upon the bracket, with a connecting piece of hard rubber. This cup is lined with lamb skin covered with silk, and contains the hard rubber electric piece which corresponds in form to the inside of the cup. A coiled covered wire connects the brass cup, with a wire attached to the burner, and terminating just above the burner.

In order to light the gas, the stop is turned, the hard rubber piece lifted partly from the cup, thus liberating the spark and lighting the gas.

The *Portable Lighter* consists of the same vase or cup, with the addition of a non-conducting handle.

When the brass cup is lifted from the electric piece and held to the conducting wire of the burner, the gas is immediately lighted.

Another portable instrument called *Double Air-Tight Electrophorus*, consists of two metallic tubes, each closed at one end, and connected together at the other, with a non-conducting ring of hard rubber, the inside being lined with lamb skin. A hard rubber rod is placed within them, the length of one of the tubes, and fitting them so as to move somewhat freely from end to end.

When the movable piece inside is allowed to fall to one end, and the tube is raised to the connecting wire of the burner, this piece changes its place again, falling into the tube held by the hand. The spark leaves the upper end of the tube at the same time and lights the gas.

The *Table Light Burner* consists of the same instrument arranged upon a pivot regularly attached to the pillar light.

Prof. J. C. Cresson exhibited some specimens of Lawrence's Metallic Paint, which he had used with very satisfactory results, both as a coating upon metals, and for making water-tight joints, and laid upon the table the following communication respecting it:—

Vivianite.—This mineral occurs sometimes with iron and copper pyrites, in which state it is found at St. Agnes, Cornwall, England. A very fine variety occurs in the gold mines at Vöröpatuck, near Siebenburg, Transylvania. It has also been found in Bavaria, in the Isle of France, and Brazil.

In this country, in two states only, has it been discovered. Mineralogists give it as a recent formation from animal matter, and their opinions are based on a variety discovered at Ballagh in the Isle of Man, accompanied by animal matter, particularly the bones of the elk and deer. At Edinburgh large quantities have been found under some old slaughter houses at the foot of the Castle Rock. In Europe it has long been used as a pigment, and preferred on both wood and metal, where the object is to protect the material from rust, rot and dampness, hence it is used largely on ships bottoms and hulls, iron railings, fences, roofs, brick walls, steam and water pipes, smoke stacks, &c., and as a paint there is none for the above named purposes to equal it.

The metallic base of the manufactured article (paint), is a combination of the peroxide and protoxide of iron with a small percentage of the peroxide of manganese.

R. Wood's cast iron Building Blocks were also exhibited. They consist of hollow shells of cast iron, having projections and holes arranged so that they may be connected together by inserting the projections of one block in the openings of the adjacent one.

By filling the shells with mortar they may be more firmly bound together; the mortar being poured into the upper row of shells, from which it flows readily to those below, through openings made for the purpose.

Mr. Thomas Shaw exhibited some samples of Homogeneous Cast Steel, made by Charles Cammell & Co. One of the specimens was a section of tube that was first flattened, and afterwards doubled over (cold), exhibiting a ductility superior to copper.

Mr. S. stated that he had seen tubes of the same metal that were pressed endwise into small cakes (not unlike collapsing a hat), without fracturing the metal.

The various uses to which this metal is applicable is obvious.

The following table of experiments made at Derby, England, January 4th and 5th, 1859, shows the comparative strength of the above metal:

EXPERIMENTS UPON HOMOGENEOUS CAST STEEL, IN COMPARISON WITH THE BEST IRONS.

No. of Experiment.	DESCRIPTION OF METAL.	SECTION.				ELONGATION.									Percentage of Elongation.	Breaking Weight in Tons.	Breaking Weight per sq. in.	Strength compared with No. 1.	SECTIONAL FRACTURE.		
		Length.	Width.	Thickness.	Area.	3	4	4½	5	5½	6	6½	7	7½					Length	Width	Area.
1	Homogeneous Steel, No. 3,	5¾	⅝	⅜	·234									½	8· 6	8	34·13	1	½	3/32	·141
2	Ditto,	“	“	“	“				⅛		3/16	7/16	⅝	1½	29·09	8	34·13	1	⅜	¼	·094
3	Homogeneous Steel, No. 2,	“	“	“	“				1/16	3/16	9/32	⅜	⅝	1	17·74	7½	32· 0	·9376	15/32	¼	·117
4	Ditto,	“	“	“	“		1/32		3/16	¼	5/16	⅜	⅝	1¼	21·74	7½	32· 0	·9376	7/16	¼	·109
5	Homogeneous Steel, No. 1,	“	“	“	“		⅛	¼	⅜	½	¾	2			35· 0	6½	27·73	·8125	7/16	3/16	·082
6	Ditto,	“	“	“	“		1/16		5/16	½	1¼				21·74	6	25· 6	·7530	½	¼	·125
7	Low Moor Iron,	“	“	“	“		1/16	3/16	5/16	½	1⅜				23· 9	6	25· 6	·7630	½	9/16	·141
8	Ditto,	“	“	“	“		⅛	3/16	⅜	⅝	1				17·74	6	25· 6	·7530	½	5/16	·160
9	Northampton Iron,	“	“	“	“		3/16	5/16	9/16	1					17·74	5½	23· 5	·6885	9/16	5/16	·175
10	Ditto,	“	“	“	“	1/32	5/32	5/16	⅝	1¾					21·74	5½	23· 5	·6885			
11	Ditto,	“	“	“	“			⅜	11/16	1⅛					19·56	5½	23· 5	·6885	½	⅓	·128
12	Jarrow Ship Plate Iron,	4½	“	5/16	·195		1/32								0· 7	4½	23· 8	·6750			
13	Ditto,	“	“	“	“										0· 0	4½	23· 8	·6750			
14	Staffordshire Iron, “Page's,”	5¾	“	⅜	·234	⅛	¼								4·34	5	21· 3	·6240			
15	Ditto,	“	“	“	“		3/16								3·26	4½	19· 2	·5624			
16	Ditto,	“	“	“	“		¼								4·34	4½	19· 2	·5626			

No. 3 hardens well.
No. 2 hardens, but not through.
No. 1 will not harden.

} These numbers do not indicate any difference in *quality*, but merely in the temper.

H. E. Thayer's improved Insulator for Telegraph Wires was exhibited. The body of the insulator is composed of a block of glass, protected by a bell-shaped casing of cast iron, through an opening in the top of which the glass projects; the casing being strongly cemented to the glass. To that portion of the glass projecting from the top of the casing is cemented a metal cap, from which projects three prongs, so arranged that the wire must be slightly bent in passing between them, it being thus prevented from slipping. The insulator is secured in its place by a pin which projects from the pole into an opening in the underside of the glass. Both the cap and casing are thus so completely insulated that there can be no communication between the wire and the pole.

The following communication from Mr. Thomas Shaw, was read, viz:

I desire to report a set of experiments by Dr. Howell, of New York, (who was appointed by the Cooper Institute) and myself, on Temperatures and Pressures; which experiments resulted in showing an error of 12½ per cent. in Regnault's scale, (the most reliable of the several scales published.) The apparatus we employed was a plain column of mercury, 40 feet high, (in order to give the pressure accurately, allowing 2·04 inches to the lb.) and a plain vertical boiler, three feet long, six inches in diameter; the heads welded on, capable of standing a pressure of 300 lbs. per square inch. The said boiler was heated at its lower end, with gas, in order to have no superheating at the top of the boiler where the thermometer was placed. The bulb of the thermometer was arranged to be immersed in the steam. There was a pipe communicating with the bottom of the boiler and the mercury column. The apparatus was operated in this wise: The boiler was filled half full of water, when the gas was ignited, heating the water and converting it into steam. The steam coming in immediate contact with the bulb of the thermometer, gave the temperature of the same, and by virtue of the pressure of the steam, forcing the water out of the pipe communicating with the mercury column, said column was elevated, thus giving the pressure. Different thermometers were used, and by three successive experiments we were forced to come to the conclusion that there was an error of 12½ per cent. in the above scale, *i. e.* one hundred lbs. by mercury column is 112½ lbs. Regnault's scale.

Several members replied to the above. They did not consider the experiments a sufficient data on which to pronounce Regnault's results to be erroneous.

A Comparison of some of the Meteorological Phenomena of FEB., 1864, *with those of* FEB., 1863, *and of the same month for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	February, 1864.	February, 1863.	February, for 13 years.
Thermometer—Highest—degree, .	57·00°	54·00°	70·00°
“ “ date, .	28th.	10th.	23d, 1860.
“ Warmest day—Mean,	49·67	46·00	59·30
“ “ “ date,	28th.	27th.	25th, 1857.
“ Lowest—degree, .	4·00	5·00	—1·00
“ “ date, .	17th.	5th.	7, ’55; 8, ’61.
“ Coldest day—Mean,	7·00	11·17	5·70
“ “ “ date, .	17th.	4th.	7th, 1855.
“ Mean daily oscillation,	15·29	13·57	13·56
“ “ “ range, .	7·18	7·54	7·30
“ Means at 7 A. M., .	30·55	30·46	29·35
“ “ 2 P. M., .	40·57	37·70	38·55
“ “ 9 P. M., .	34·74	34·04	33·53
“ “ for the month,	35·29	37·07	33·81
Barometer—Highest—Inches, .	30·375 in.	30·671 in.	30·671 in.
“ “ date, . .	19th.	4th.	4th, 1863.
“ Greatest mean daily press.	30·267	30·537	30·595
“ “ date, . .	18th.	4th.	12th, 1857.
“ Lowest—Inches, .	29·284	29·345	29·065
“ “ date, . .	16th.	20th.	23d, 1853.
“ Least mean daily press.,	29·383	29·501	29·227
“ “ date, . .	16th.	20th.	16th, 1856.
“ Mean daily range, .	0·182	0·265	0·221
“ Means at 7 A. M., .	29·792	30·031	29·919
“ “ 2 P. M., .	29·742	29·977	29·871
“ “ 9 P. M., .	29·796	30·024	29·904
“ “ for the month, .	29·777	30·011	29·898
Force of Vapor—Greatest—Inches,	0·262 in.	0·322 in.	0·549 in.
“ “ “ date, .	1st.	6th.	16th, 1857.
“ “ Least—Inches, .	·022	·027	·013
“ “ “ date, .	17th.	4th.	6th, 1855.
“ “ Means at 7 A. M.,	·132	·142	·139
“ “ “ 2 P. M.,	·124	·149	·159
“ “ “ 9 P. M.,	·139	·146	·157
“ “ “ for the month,	·132	·146	·152
Relative Humidity—Greatest—per ct.,	90 per ct.	100 per ct.	100 per ct.
“ “ “ date,	6th.	19th.	Often.
“ “ Least—per ct.,	20·0	29·0	20·0
“ “ “ date, .	22d.	16th.	22d, 1864.
“ “ Means at 7 A.M.,	70·1	77·3	78·7
“ “ “ 2 P.M.,	46·6	63·5	63·2
“ “ “ 9 P.M.,	63·8	70·9	75·3
“ “ “for the month	60·2	70·5	72·4
Clouds—Number of clear days,* .	10	7	8·1
“ “ cloudy days, .	19	21	20
“ Means of sky cov’d at 7 A.M.,	50·7 per ct.	73·6 per ct.	61·6 per ct.
“ “ “ “ 2 P.M.,	63·1	62·9	61·0
“ “ “ “ 9 P.M.,	47·6	63·9	47·2
“ “ “ for the month,	53·8	66·8	56·6
Rain and melted Snow—Amount, .	0·697 in.	3·824 in.	2·730 in.
No. of days on which Rain or Snow fell,	5	13	10
Prevailing Winds—Times in 1000,	s80°41′ w·379	n56°19′w·139	n72°59′w·283

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* Less than one-third covered at the hours of observation.

A Comparison of some of the Meteorological Phenomena of the WINTER *of* 1863–64, *with that of* 1862–63, *and of the same Season for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By J. A. KIRKPATRICK, A. M.

	Winter, 1863–64.	Winter, 1862–63.	Winter for 13 years.
Thermometer—Highest—degree,	65·00°	64·00°	71·00°
“ “ date,	Jan. 29th.	Dec. 15th.	Dec. 2, 1859.
“ Warmest day—Mean,	54·17	58·33	62·80
“ “ “ date,	Dec. 13th.	Jan. 15th.	Dec. 19, 1856.
“ Lowest—degree,	4·00	5·00	—5·50
“ “ date,	Feb. 17th.	Feb. 5th.	Jan. 23, 1857.
“ Coldest day—Mean,	7·00	11·17	—1·00
“ “ “ date,	Feb. 17th.	Feb. 4th.	Fe. 7,’55; Fe. 8,’61
“ Mean daily oscillation,	14·10	13·07	12·64
“ “ “ range,	6·52	6·53	6·72
“ Means at 7 A. M.,	30·44	32·22	29·74
“ “ 2 P. M.,	39·06	39·47	37·83
“ “ 9 P. M.,	33·89	35·13	33·19
“ “ for the Winter,	34·46	35·61	33·59
Barometer—Highest—Inches,	30·495 in.	30·671 in.	30·704 in.
“ “ date,	Dec. 7th.	Feb. 4th.	Jan. 28, 1853.
“ Greatest mean daily press.	30·423	30·553	30·611
“ “ date,	Dec. 7th.	Jan. 18th	Dec. 18, 1856.
“ Lowest—Inches,	29·167	29·127	28·941
“ “ date,	Dec. 14th.	Jan. 16th.	Jan. 23, 1853.
“ Least mean daily press.,	29·341	29·298	29·086
“ “ date,	Dec. 14th.	Jan 16th.	Jan. 23, 1853.
“ Mean daily range,	0·204	0·243	0·217
“ Means at 7 A. M.,	29·892	29·963	29·949
“ “ 2 P. M.,	29·841	29·913	29·906
“ “ 9 P. M.,	29·897	29·955	29·935
“ “ for the Winter,	29·877	29·944	29·930
Force of Vapor—Greatest—Inches,	0·486 in.	0·462 in.	0·551 in.
“ “ “ date,	Dec. 14th.	Jan. 16th.	Dec. 2, 1859.
“ “ Least—Inches,	·022	·027	·013
“ “ “ date,	Feb. 17th.	Jan. 4th.	Feb. 6, 1855.
“ “ Means at 7 A. M.,	·136	·154	·139
“ “ “ 2 P. M.,	·139	·162	·160
“ “ “ 9 P. M.,	·141	·160	·153
“ “ “ for the Winter,	·139	·159	·151
Relative Humidity—Greatest—per ct.,	100 per ct.	100 per ct.	100 per cent.
“ “ “ date,	Jan. 18th.	Ja. 21, Fe. 19.	Often.
“ “ Least—per ct.,	20·0	29·0	20·0
“ “ “ date,	Feb. 22d.	Feb. 16th.	Feb. 22, 1864.
“ “ Means at 7 A.M.,	72·4	77·9	78·5
“ “ “ 2 P.M.,	54·7	63·4	65·5
“ “ “ 9 P.M.,	67·3	73·5	75·6
“ “ “ for Winter,	64·8	71·6	73·2
Clouds—Number of clear days,*	33	27	26·2
“ “ cloudy days,	58	63	64·1
“ Means of sky cov’d at 7 A.M.	55·6 per ct.	64·7 per ct.	62·8 per ct.
“ “ “ “ 2 P.M.	57·6	63·9	62·2
“ “ “ “ 9 P.M.	45·5	55·7	47·3
“ “ “ for Winter,	52·9	61·4	57·4
Rain and Melted Snow—Amount,	7·446 in.	10·077 in.	9·598 in.
No. of days on which Rain or Snow fell,	25	35	30·6
Prevailing Winds—Times in 1000,	N85°5′W·306	N68°38′W·199	N64°20′W·295

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* Less than one-third covered at the hours of observation.

JOURNAL

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APRIL, 1864.

CIVIL ENGINEERING.

Description of Lighthouses lately erected in the Red Sea. By W. PARKES, M. Inst.C.E.

From the London Civ. Eng. and Arch. Jour., Dec., 1863.

HAVING been instructed by the Board of Trade to make the necessary preliminary surveys for establishing lights to facilitate the navigation of the northern portion of the Red Sea, the author recommended three sites; 1st. Zafarana Point on the Egyptian shore, 50 miles from Suez; 2d. Ushruffee reef, on the western side of the navigable channel of the Straits of Jubal, 150 miles from Suez; and 3d. the Dædalus reef in the centre of the Red Sea, 350 miles from Suez, and 180 miles from the entrance of the Gulf of Suez. These sites having been approved by the Egyptian government, by the Board of Trade, and by the Directors of the Peninsular and Oriental Steam Packet Company, the immediate execution of the works was authorized, upon designs submitted by the author.

Zafarana Point being on the mainland, it was considered most advantageous to construct a tower and light-keepers' dwellings of rubble stone, and to employ native labor entirely, under the direction of H. E. Linant Bey, the chief engineer of the public works department of the Egyptian government. The design presented no feature calling for special remark, and the works had been carried out in a very satisfactory manner. The light was a fixed dioptric of the first order, visible, over five-eighths of a circle, at a distance of 14 miles. It was first exhibited on the 1st of January, 1862.

The main features of the other sites were then described. The Ushruffee was a coral reef, of which the sides sloped irregularly from the level of a few inches under low-water to a depth of from 8 to 10 fathoms, no part being above the water, and there being very little sand, even in the cavities of the coral. The Dædalus reef was a submerged island with a flat top of an oval form, 1200 yards in length and 450 yards in breadth, the sides being generally vertical, or in some places even overhanging. The actual surface of the coral was about 6 inches under low-water: but there was a small shifting bank of sand near the south-east end, which was dry at low-water, and sometimes also to a small extent at high-water. The range of tide was about 2 feet at springs.

The peculiar conditions which had to be considered in designing the proposed constructions were—First, the force of the sea would be completely broken at some distance within the edge of the reef. Secondly, the structures would have to be built upon the surface of the reef, and not be sunk into it, as no additional security could be thus obtained, while the advantage of the natural platform would be lost. Thirdly, in the absence of any definite experience as to the actual weight which the reefs would bear without being crushed, it was desirable to keep the weight per superficial foot of foundation as low as possible. Fourthly, the buildings had to be designed so as to mitigate the intense summer heat. And fifthly, in the case of the Dædalus, as the materials would have to be brought from Suez, and as there was no anchorage, it was necessary that a steamer should be employed capable of keeping close to the reef in any wind, and of providing quarters for the workmen and storage room for the materials, until a proper depôt and habitation could be formed on the reef itself. These requirements rendered it essential that the materials should be small in bulk, that the several parts should be light and easily handled, and that the mode of putting them together should be as simple as possible.

It appeared to the author that these conditions would be best attained by adopting a structure of wrought iron supported on teak piles, resting on, and the feet bedded in, a layer of concrete, so as to distribute the weight, the surface of the concrete being a little above the level of high-water; and that, by filling in the wrought iron framework with strong corrugated iron, so as to form a series of rooms one above the other, as a central column, with verandas or galleries around each room, likewise partially enclosed, a portion at least of the sun's rays would be prevented from falling on the walls of the rooms, whilst there would be a free admission of air.

As the Dædalus light had only to guard against the dangers of the reefs on which it was placed, it was not necessary that that structure should exceed the limited height that would allow of four tiers of rooms and of accommodation for the lighting arrangements. These together brought the light to an elevation of 62 feet above the mean level of the sea. As the Ushruffee light had to lead vessels past dangers 12 or 14 miles distant, a more powerful light at a greater elevation was required. The height fixed upon was 125 feet above the mean level of the sea. The frame-work was of the same description in both cases, but in the latter case there were eleven tiers, whereas in the Dædalus there were only four tiers. Details were then given of the Ushruffee structure, as being the larger of the two. It was stated that this structure was supported upon eighteen piles, each 18 feet long and 18 inches diameter, arranged in two concentric circles. The inner circle consisted of six piles and was 15 feet diameter, while the outer circle was formed of six pairs of piles (the piles of each pair being 6 feet apart), 37 feet diameter at the top, and the feet spread outwards at a batter of 1 in 12. The feet of the piles rested upon the natural surface, and there were shoulders on each side of the piles resting on sleepers of teak, bedded on the concrete 2 to 3 feet above the surface of the coral. The heads of the piles passed through circular wrought iron collars, to which they were accurately fitted, and any loosening by the shrinkage of the timber was provided for, by fitting a number of wedges of greenheart timber into corresponding grooves in the pile heads, in which they could be driven down when slack. A direct bearing surface was also given by iron screws 2 inches diameter, which passed through each collar, and entered 2 inches into the wood. The collars had projecting arms to which the bottom framing was riveted. The superstructure consisted of a repetition of three main parts, which might be called respectively standards, cills, and radiators. In each tier there were 24 standards, arranged in two concentric circles, and these were connnected at the top and bottom by cills, thus forming two twelve-sided polygons, the corresponding angles of which were connected by the radiators. With the exception of a few parts near the bottom, no separate piece exceeded 4 cwt. in weight. The floors of the rooms were composed of cast iron plates covered with concrete, and from the lowest floor there was suspended a hemispherical water tank, capable of holding 1500 gallons. The floor of the galleries were formed of open cast iron gratings. The whole of the iron work of the structure was erected on Messrs. Forrester and Co.'s premises, at Liverpool, with the view of attaining the accurate fitting of the parts, and of testing its strength. While at its full height, with the exception of the piles at the bottom and the lantern at the top, but with the joints merely bolted together, it was exposed, in September and October, 1860, to two gales of wind of the registered force, respectively, of 20·5 lbs. and 24·3 lbs. per sq. ft. There was no appearance of any straining of the joints, and a careful examination failed to discover any swaying movement at the top, though there was a sensible vibration. Since its erection on the reef it had been subjected to two severe gales (in June 1862, and in January 1863), with similar results, for although the top of the building had vibrated, there were no symptoms of straining having occurred.

The general principles of the construction of the Dædalus lighthouse were the same as those of the Ushruffee, with such modifications as its smaller size demanded; thus there were only twelve instead of eighteen piles, and the dimensions of the parts of the frame-work were the same as for the upper piers of the Ushruffee. The building was stated to be very stiff, scarcely any vibration being perceptible under a strong breeze.

The lanterns were of the same construction for the three lighthouses. Those for the Ushruffee and the Zafarana, being for first-order lights, were identical in every respect. That for the Dædalus being for a second-order light, was of reduced size. In construction they were similar to those generally manufactured for the Trinity Corporation, but with special arrangements for mitigating the powerful effects of the sun. At the suggestion of Professor Faraday, a wind-guard was substituted for the ordinary revolving vane and cowl. The lanterns and light apparatus were furnished by Messrs. Wilkins and Co., the optical portion having been manufactured by Messrs. Chance Brothers, whose improvements in lighthouse illumination deserved special notice. The Zafarana and the Dædalus were fixed lights, while the Ushruffee had a revolving light frame.

The whole of the materials of the two iron lighthouses, and of the three lanterns and light apparatus, were, with some trifling exceptions, despatched from Liverpool within ten months from the date of the author receiving instructions to proceed to survey the sites, at a distance of nearly 3000 miles from Great Britain.

As the structures were designed with a special view to the peculiar circumstances of the sites upon which they were to be erected, it was considered desirable to give some account of the operation of erection. The Peninsular and Oriental Steam Packet Company granted, gratuitously, the services of the " Union," a screw steamer of 300 tons, and 60-horse-power, the Egyptian government paying for all wages, stores, and coal. The materials arrived at Alexandria on the 12th of November, 1860, but it was not until the 20th of December following that they were all received at Suez, and placed on board the "Union," which then sailed for the Ushruffee reef. The expedition thus commenced was, unfortunately, not successful. The causes of the failure were given at length in the Paper, but it would be sufficient to state the results of the season's labor, which lasted three months. The piles were erected on the reef and their heads were connected by the bottom iron frame. The whole of the iron-work was landed, and laid out in order, upon one of the neighboring islands. This was not originally intended, as the plan decided upon was to moor the ship as near as possible to the reef during the progress of the works, and to land and sort the materials upon the concrete base of the lighthouse itself. After a full inquiry into the circumstances which had led to this failure, the author was instructed to make arrangements for a new expedition, and he readily assented to the suggestion that he should remain on the spot until the operations as to which difficulty had to be anticipated were completed. The permanent superintendence was intrusted to the late Mr. C. W. Scott (Assoc. Inst. C.E.), Captain W. Kirton being in command of the " Union." As the materials had been landed on the Ushruffee Island, it was determined to form a land establishment for the working party there rather than to quarter the men on board. This left the steamer free to carry a working party to the Dædalus, without interfering with the operations on the Ushruffee. The staff was re-organized, and the list of plant and of materials revised; but owing to several causes the operations were not resumed on the Ushruffee until the 8th November, 1861. The first step was to form the shore establishment. This occupied three weeks, owing to the want of skill of the native carpenters in getting the huts ready for occupation. During this time, however, some progress had been made at the lighthouse. The caisson of iron plates to enclose the concrete base had been set up, and about 200 tons of gravel had been placed upon the reef, where it was exposed to a wash sufficient to remove some of the clayey particles. The process of depositing the concrete was then commenced, the plan adopted being to deposit it upon sheets of tarpaulin, which sunk with the weight, and protected the soft material from the action of the water, until a mass of several tons was collected. When the whole space was covered in this way up to above low-water mark, the remainder was deposited, as the state of the tide allowed, until the whole height of 5 feet from the surface of the coral was complete. The first half of the concrete was formed of 6 measures of gravel to 1 of Portland cement, mixed in the lighters moored alongside the caisson. The second half was formed of lime, puzzuolano, and broken stone, in the manner usually practised in the country. The latter was mixed dry at Suez, and was wetted on being deposited.

When the success of the process of depositing the concrete was well assured, the author turned his attention to the Dædalus, which was reached on the 26th of December. A site for the lighthouse was chosen near a small sank-bank of triturated coral, as it was determined to use the sand for the concrete, and as it was also convenient for beaching boats, &c. The surface of this reef was very irregular, there being numerous hollow places, varying from 1 ft. 6 in. to 2 ft. in depth. A four-legged shears was set up on the intended site, and a platform fixed upon it at the level of the underside of the pile collars. Upon this platform were bolted together the plates forming the polygon to complete the inner circle of piles. The six piles were then successively raised on end, and the collars were bolted to the polygon. When the six piles were fixed, the original stage was removed, and a new one was formed to receive in a similar manner the outer polygon. This having been fixed in place, and the two polygons connected by the radiators, the outer piles were raised. The caisson plates were then set up, being first bolted together and then partially riveted. The whole of these operations occupied seven days and a quarter, the staff employed consisting of four workmen, and parties of from six to eight men of the crew. The lighter was then moored upon the reef, and the steamer proceeded to Ushruffee, whence it returned with 50 tons of cement and twenty Arab laborers, under a native foreman, for depositing the concrete, and having a second lighter in tow. The concrete here used consisted of 3½ measures of coral sand to 1 measure of cement. The first operation was to fill up the holes between the coral lumps with coal bags filled with concrete, so as to make an even surface to lay the tarpaulins upon. The deposit was then carried on in the same manner as at the Ushruffee. The quantity of cement above alluded to was only sufficient to complete three-fourths of the required height of the block. This occupied just eight days, and then the author returned to Ushruffee, and finding five tiers of that lighthouse erected and partly riveted, he handed over the entire charge of both works to Mr. Scott, who shortly after proceeded with a party of four mechanics, two riveters, four laborers, and ten Arabs, in all twenty, besides the crew, to the Dædalus. With these, in twenty-six days—the work being carried on only on twenty-one days—the whole frame-work was erected and riveted together, two floors and the water-tank were completed, and the lower room was enclosed. Two mechanics and four seamen were then placed in the building, with provisions and water, to continue the work, and the "Union," with Mr. Scott, and the remainder of the working party, returned to Ushruffee. Thus in thirty-seven working days, the main portion of the building, now 57 feet in height, was so far finished as to be habitable for a party sufficient to complete the remaining details. Had the ship been of larger burthen, it was believed that these thirty-seven days might have been continuous, and that the whole would have been accomplished during an absence of about seven weeks from Suez. The author referred in terms of the highest praise to the manner in which what might be termed the nautical part of the undertaking had been carried out by Captain Kirton. Subsequently the materials for the lantern and the lighting apparatus were deposited in the building, and afterwards one leading mechanic, two laborers, and five seamen, completed the work between November, 1862, and the 1st of February, 1863, when the light was exhibited.

At the last mention of the Ushruffee the concrete base had been completed, and five out of eleven tiers of framing had been erected. During the absence of Mr. Scott and his party at the Dædalus, a European boat's crew of six men was engaged in conveying the remainder of the materials to the reef and in sorting them there. On the return of the working party the erection was rapidly proceeded with, in the face of much difficulty, from almost constant high northerly winds. The time actually taken in erecting the skeleton frame-work, 106 feet in height, was two months, and the riveting was completed within three months. The two succeeding months were occupied with the erection of the lantern and lighting apparatus, and completing the details of the building, and on the 1st of July, 1862, the light was first exhibited.

In connexion with this undertaking the profession has to regret the loss of a very promising young member, Mr. C. W. Scott, who towards the close of the operations was attacked with symptoms which developed the seeds of a disease of long standing, under which he succumbed after an interval of five months.

With regard to the cost, a mere statement of the total amount expended upon the two lighthouses, £55,211 in all, would convey an erroneous impression, unless accompanied by an explanation. The cost of the whole as an engineering work, independently of the employment of the steamer, might be taken at £32,079 including all contingencies. Supposing the reef to have been within a boating distance, say $1\frac{1}{2}$ mile and $\frac{1}{2}$ mile from Suez, if the steamer had been equipped at Suez, and had been continuously employed, then, on this supposition, the cost might have been £42,082. The remaining expenditure, £13,029 was entirely exceptional, arising mainly from the steamer being equipped at Bombay. Proc. Inst. Civil Eng., Nov. 10, 1863.

For the Journal of the Franklin Institute.

Trussed Arch.

By De Volson Wood, Prof. of C. E. University of Michigan.

The problem which I propose to discuss may be stated as follows:

Let the arch of the Truss be a parabola, or if it be polygonal, let the vertices of the polygon be in a parabola; the tie which joins the ends of the arch be horizontal; all the parts of the truss be reduced to mathematical lines, and the joints perfectly flexible. Let the load over any part of the truss be uniform—or, what is better, let the weights upon the joints equal each other.

It is well known that for an uniform load over the whole length of a parabolic arc, there are no strains in it but compression (or tension), and hence, if the load be above the arch there will be no strains upon the ties and braces; and if the load be below, the ties will simply sustain the total load; hence the strains upon the several parts are easily computed. I will, therefore, proceed to the case of a partially loaded truss. Let the horizontal tie be divided into equal parts by the trussing, and let each part be called a bay.

Let N = total number of bays in the horizontal tie,

n = the number of a bay which corresponds to the number of a brace or pair of braces,

F = the force of compression in the arch at any point,

$F_1 = H$ = horizontal force in the tie,

F_2 = the strain on a brace or tie in the truss,

i = the inclination of the arch to the horizontal,

θ = the inclination of a brace, or tie, to the vertical,

D = greatest depth of the truss = distance from the vertex of the parabola to the horizontal tie,

h = depth of the truss at any point,

l = length of bay,

p = one of the equal weights which constitutes the load,

V and V_1 = vertical re-actions of the supports, and let n be counted from the V support,

x be horizontal and positive towards V,

y be vertical and positive downwards, then if θ is on the right of y it will be positive; if on the left, negative.

x', y' be the co-ordinates of c,

x'', y'' be the co-ordinates of b,

$2p_1$ the parameter of the parabola.

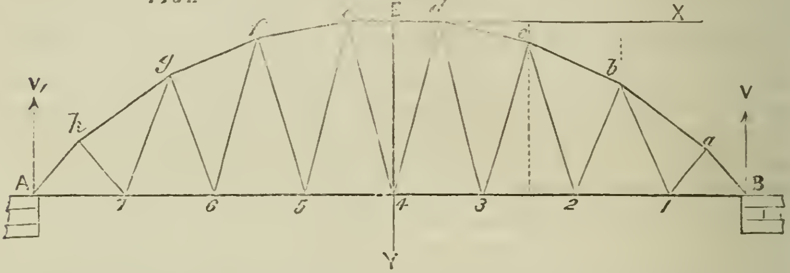
For the equation of the arch we have,

$$x^2=2p_1y \text{ or } \tfrac{1}{4}N^2l^2=2p_1D \quad \therefore\ 2p_1=\frac{N^2l^2}{4D} \text{ which gives}$$

$$x^2=\frac{N^2l^2}{4D}y \text{ or } y=\frac{4D}{N^2l^2}x^2 \quad . \quad . \quad . \qquad (1)$$

It can easily be shown that if the truss be uniformly loaded from any brace to the remote end, the strain upon the brace will be greater than if there be an additional uniform load between it and the near end. I shall therefore consider the case of such an uniform load.

FIG. 1.



First, take the case of triangular trussing as shown in Fig. 1. Let equal weights rest upon the joints *c*, *d*, *e*, *f*, *g*, and *h*, and none upon *a* and *b*. This will insure, as stated above, a greater strain on *c*2 than if *b* or *a*, or both were loaded with the same weights. Suppose now that a vertical section is made just at the right of *c*; said section will intersect *cb*, *c*2, and 3 2, and the strains in these bars must be in equilibrium with the forces between the section and B; in other words they must be in equilibrium with V_1, and since V acts vertically, we have the vertical components in *cb* and *c*2 equal V, and the horizontal components in the same bars equal the strain on 3 2, or equal H. Hence using the notation given above, we readily have

$$F\sin i+F_2\cos\theta=V \quad . \quad . \quad . \qquad (2)$$

$$F\cos i+F_2\sin\theta=H \quad . \quad . \quad . \qquad (3)$$

Multiply (2) by cos *i*, (3) by sin *i*, subtract and reduce gives

$$F_2\cos\theta=\frac{V-H\,\text{tang}\,i}{1-\text{tang}\,\theta\,\text{tang}\,i} \quad . \quad . \quad . \qquad (4)$$

This formula is general, whatever be the curve of the arch.

Calling *a* 1 *b* the first pair of braces (or ties) counting from B; let *b* 2 *c* be the n^{th} pair; then will

2 1 be the n^{th} bay; 3 2 the $(n+1)^{th}$ bay, and

$N-n=$the number of loaded joints,

$\therefore\ (N-n)p=$the total load on the truss,

$Nl=AB=$total length of truss,

$\frac{1}{2}(N-n)l=$distance from the centre of the load to A, Then by the principle of the lever we have

$$\mathrm{V}\mathrm{N}l=(\mathrm{N}-n)\,p\,.\,\tfrac{1}{2}\,(\mathrm{N}-n)\,l$$

$$\therefore\quad \mathrm{V}=\frac{(\mathrm{N}-n)^2}{2\,\mathrm{N}}\,p. \qquad . \qquad . \qquad . \qquad . \qquad (5)$$

We also have from the figure,

$$\mathrm{B}\,m=(n+\tfrac{1}{2})l$$

$$x'=\mathrm{EK}=(\tfrac{1}{2}\,\mathrm{N}-n-\tfrac{1}{2})l \;\therefore\; \text{By (1) we have } y'=\frac{\mathrm{D}}{\mathrm{N}^2}\,(\mathrm{N}-2\,n-1)^2$$

$$x''=\mathrm{EL}=(\tfrac{1}{2}\,\mathrm{N}-n+\tfrac{1}{2})l \qquad . \qquad . \; y''=\frac{\mathrm{D}}{\mathrm{N}^2}\,(\mathrm{N}-2n+1)^2$$

$$\text{tang}\, i=\frac{y''-y'}{l}=\frac{4\,\mathrm{D}}{\mathrm{N}^2 l}\,(\mathrm{N}-2\,n) \qquad . \qquad . \qquad . \qquad (6)$$

$$\text{tang}\, \theta=\frac{\tfrac{1}{2}\,l}{\mathrm{D}-y'}=\frac{l\,\mathrm{N}^2}{2\,\mathrm{D}\left[\mathrm{N}^2-(\mathrm{N}-2\,n-1)^2\right]} \qquad . \qquad . \qquad . (7)$$

To find the tension in the bar 3 2, we take the origin of moments at *c*, and we readily have

$$\mathrm{H}\,(\mathrm{D}-y')=\mathrm{V}\,(n+\tfrac{1}{2})\,l$$

$$\therefore \mathrm{H}=\frac{\mathrm{N}^2 l\,\mathrm{V}}{2\,\mathrm{D}\,(2\,\mathrm{N}-2\,n-1)} \qquad . \qquad . \qquad (8)$$

Substitute (5), (6), (7), and (8) in (4), gives

$$\mathrm{F}_2 \cos\theta=\frac{(\mathrm{N}-n)^2 p}{2\,\mathrm{N}}\left[\frac{4n^2-1}{4n\,\mathrm{N}-4n^2-1}\right]$$

$$=\frac{(\mathrm{N}-n)}{2\,\mathrm{N}}\;p\,\left[n-\frac{\mathrm{N}-2n}{4n\,(\mathrm{N}-n)-1}\right] \qquad . \qquad (9)$$

We see in (9) that when n is less than $\tfrac{1}{2}$ N the fraction in the parenthesis is negative; but when it exceeds $\tfrac{1}{2}$ N, it becomes positive, and observing that when it is greater than $\tfrac{1}{2}$ N, less than one-half the bridge is loaded, we have this peculiar result: the strain upon *a* tie or brace, is greatest when the truss is loaded between it and the nearer end. We may also observe, that in practical cases the omission of the fraction in the parenthesis, will not give an error of more than one-fifth the true value, and generally the error will be much less. Hence we have approximately

$$\mathrm{F}_2 \cos\theta=\frac{(\mathrm{N}-n)\,np}{2\,\mathrm{N}}\;{}^{*} \qquad . \qquad . \qquad . \qquad (10)$$

If (10) were true we observe that the strains on any brace will be the same if the load extend from it to either end.

It is easy to show, geometrically, that the vertical components of the strains on each of the braces which constitute a pair, as has been designated, are equal. To give a further application of the analysis, I will prove it by equation (4). Now let a vertical section be made, just at the left, but infinitely near *b*. The section will cut *b* 2, *bc*, and

---

[*] Bow in his excellent Treatise on Bracing, gives this as the *exact* formula.

1 2. Tang i and V, will remain the same, but tang θ is negative and equal $\frac{\frac{1}{2}l}{\text{D}-y''}$; . . . - (10′)

and to find H we have $\text{H}\,(\text{D}-y'')=\text{V}\,(n-\frac{1}{2})\,l$.

$$\therefore\quad \text{H}=\frac{\frac{1}{2}(2\,n-1)\,l}{\text{D}-y''}\text{V} \qquad . \qquad . \qquad . \qquad (11)$$

These in (4) give

$$\text{F}_2\cos\theta=\text{V}\,\frac{1-\dfrac{2n-1}{\text{D}-y''}\cdot\dfrac{2\,\text{D}}{\text{N}^2}(\text{N}-2n)}{1+\dfrac{2\,\text{D}}{(\text{D}-y'')}\cdot\dfrac{(\text{N}-2n)}{\text{N}\,2}} \text{ which reduced gives}$$

$$\text{F}_2\cos\theta=\frac{(\text{N}-n)^2}{2\,\text{N}}\,p\quad\left[\frac{4n^2-1}{4n\text{N}-4n^2-1}\right] \text{ the same as before.}$$

But the strains on the braces of a pair will not be equal, for they are unequally inclined. To find F_2, we find θ from (7) and use it in (9).

Example.—Let $\text{N}=8$ and $\text{D}=l$.

To show more clearly the value of the fraction in the parenthesis, Eq. (9), I will keep it separate in the following table:

No. of the pair of braces, or $n=$	Vertical component of the strains on the n^{th} pair; or $\text{F}_2\cos\theta$, Eq. (9.)	Inclination of the braces; or θ, Eq. (7.)	Values of $\cos\theta$.	Numbers in the second column divided by those in the fourth: or, F_2.
0	$0+\frac{8}{2}\quad p=4{\cdot}000\,p.$	*Ba*, 64°53′	0·4245	9·422 p
1	$(\frac{7}{16}-\frac{7}{72})\quad p=0{\cdot}3403\,p.$	*a*1, 64°54′ *b*1, 39°22′	0·4245 0·7731	0 802 p 0·440 p
2	$(\frac{3}{4}-\frac{3}{94})\quad p=0{\cdot}7189\,p.$	*b*2, 39°22′ *c*2, 30°11′	0 7731 0 8644	0 928 p 0·831 p
3	$(\frac{15}{16}-\frac{5}{472})\;p=0\,9270\,p.$	*c*3, 30°11′ *d*3, 26°55′	0·8644 0·8916	1·072 p 1·038 p
4	$p=1{\cdot}0000p.$	*d*4, 26°55′ *e*4, 26°55′	0·8916 0·8916	1·121 p 1·121 p
5	$(\frac{15}{16}+\frac{3}{472})p=0{\cdot}9438\,p.$	*e*5, 26°55′ *f*5, 30°11′	0·8916 0·8644	1·057 p 1·090 p
6	$(\frac{3}{4}+\frac{1}{94})\quad p=0{\cdot}7606\,p.$	*f*6, 30°11′ *g*6, 39°22′	0·8644 0·7731	0·879 p 0 998 p
7	$(\frac{7}{16}+\frac{1}{72})\quad p=0{\cdot}4514\,p.$	*g*7, 39°22′ *h*7, 64°54′	0·7731 0·4245	0·583 p 1·060 p
8	$p=0{\cdot}000\,p.$			

We may readily conceive these strains to be produced by an uniform load moving without shock over the truss from B to A; and the same strains in a reverse order may be produced by a movement in the opposite direction.

When N is even we may have $n=\frac{1}{2}\text{N}$, which in (9) gives $\text{F}_2\cos\theta=\frac{1}{8}\text{N}p$.

Next suppose the load on the horizontal tie. This is the more natural place for the surcharge. In this case the vertical force sustained by each pair when they are all equally loaded, is p. But if only a portion of the truss be loaded, equation (9) will not apply, as may be seen from the following statements. To produce the greatest strain on $c3$, we unload joints 1 and 2, and load 3, 4, 5, 6, and 7; and calling the bay $2-1$, the n^{th}, the load will be $(\mathrm{N}-n)p$; and the centre of the loading will be $\frac{1}{2}(\mathrm{N}-n+1)\,l$ from A; hence, to find V we have

$$\mathrm{V}\,.\,\mathrm{N}l=\tfrac{1}{2}(\mathrm{N}-n)(\mathrm{N}-n+1)l.$$

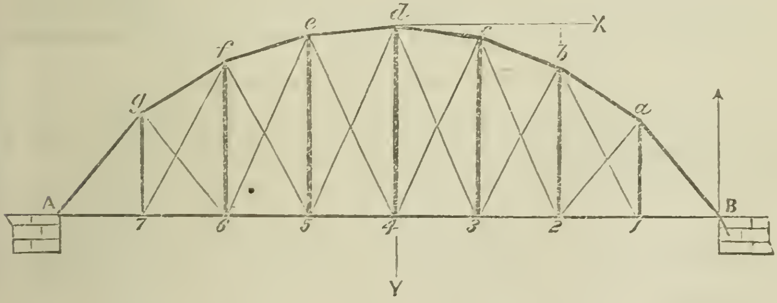
$$\therefore\ \mathrm{V}=\frac{(\mathrm{N}-n)(\mathrm{N}-n+1)l}{2\mathrm{N}}. \qquad . \qquad . \qquad (12)$$

If joint 2 be loaded, the vertical force on $c2$ will not be the same as on $b2$, but we may find it on $b2$ by making a section just to the left of b; and substitute in (4) the values of (6), (10′), and (12). But we observe that these values are all the same as those before used, except V; hence we have at once, for the strain on the first of the n^{th} pair,

$$\mathrm{F}_2\cos\theta=\frac{(\mathrm{N}-n)(\mathrm{N}-n+1)}{2\,\mathrm{N}}\,p\left[\frac{4n^2-1}{4n\mathrm{N}-4n^2-1}\right] \qquad (13)$$

Next consider panel trussing as shown in Fig. 2, and let the load be upon the lower chord, and let the bays in the lower chord be of equal lengths. It will make some difference in the strains whether they be resisted by ties or braces.

FIG 2.



First consider braces.

Let x' and y' be the co-ordinates of c.

x'' y'' be the co-ordinates of b.

3 2 be the n^{th} bay, and call $c2$ the n^{th} brace, and

$e3$ the n^{th} tie. Suppose the load is on from A to 3, and off from B to 2, including 2; then the load is $(\mathrm{N}-n)p$; and V the same as (12). The equation of the curve is given by (1), hence we have

$$x'=(\tfrac{1}{2}\mathrm{N}-n)l\ ,\ y'=\frac{\mathrm{D}}{\mathrm{N}^2}(\mathrm{N}-2n)^2$$

$$x''=(\tfrac{1}{2}\mathrm{N}-n+1)l\ ,\ y''=\frac{\mathrm{D}}{\mathrm{N}^2}(\mathrm{N}-2n+2)^2$$

$$\text{tang}\ i = \frac{y''-y'}{l} = \frac{4\text{D}}{l\text{N}^2}(\text{N}-2n+1) \quad . \quad . \quad . \quad (13a)$$

$$\text{tang}\ \theta = \frac{l}{\text{D}-y'} = \frac{l\text{N}^2}{4\text{D}n(\text{N}-n)} \quad . \quad . \quad . \quad (14)$$

Now conceive a section made just at the right, but infinitely near c, so as to cut cb, $c2$, and 3 2; then will equation (4) be applicable. It cuts $b3$, but it is not in action when $c2$ is. To find H, we have by an equation of moments,

$$\text{H}\,(\text{D}-y') = \text{V}nl$$

$$\therefore \text{H} = \text{V}\frac{l\text{N}^2}{4\text{D}(\text{N}-n)}$$

These in (4) give by reduction

$$\text{F}_2 \cos\theta = \frac{(\text{N}-n)(\text{N}-n+1)(n-1)n}{n\text{N}-\text{N}-n^2+2n-1} \cdot \frac{p}{2\text{N}} = \frac{(\text{N}-n)\,n}{2\text{N}}p \quad . \quad (15)$$

This is a maximum for $n = \frac{1}{2}\text{N}$, for which it equals $\frac{1}{8}\text{N}p$; hence, if there are less than N bays the central brace will not be strained as much as for half the truss loaded as for the whole truss loaded; if N $= 8$, it will be strained the same; if greater it will be strained more for half the truss loaded than for the whole loaded. We also see that for the partial load the central ones are strained more than the end ones. The formula does not give the vertical strain on Ba; for it is really $\text{V} = \frac{1}{2}(\text{N}-1)p$; but for $n=1$, (15) gives $\frac{\text{N}-1}{2\text{N}}p$. The reason of this failure will be found by observing that in making the reduction of (15), a factor $(n-1)$ is cancelled in both terms of the fraction, which factor is 0 for $n=1$, which would make the fraction $\frac{0}{0}$.

Example.—Let $\text{N} = 8$. D has disappeared in the reduction, but it must be known to get θ. Let $\text{D} = 2l$. We have

No. of the brace, or $n=$	Vertical component of the strain on the n^{th} brace, or $\text{F}_2 \cos\theta$; eq. (15)	Inclination of the braces, or θ, eq. (14)	Values of $\cos\theta$.	Strains on the n^{th} brace, or F_2 eq. (15)
1 or 7; or g 6	$\frac{7}{16}p$	48°48′	0·6587	0·6641 p
2 or 6; or $b1$ or f 5	$\frac{12}{16}p$	33°41′	0·8321	0·9013 p
3 or 5; or $c2$ or $e4$	$\frac{15}{16}p$	28° 4′	0·8824	1·0624 p
4 ; or $d3$	1 p	26°34′	0·8944	1·1182 p

Although the braces which we have considered, at the rear end of the load incline towards the load, yet they must incline both ways as in the figure to completely brace the truss. For this partial load, the vertical strain on $a1$, is the same as on $b1$; and if the load extends from 3 to A, the vertical forces on $c2$ and $b2$ are equal. Similarly for the other pairs; hence, observing the numbers in the second column of the preceding table, and we have for the actual strains on the vertical ties taken in their order from either end, when the load extends from the tie to the other end: $\frac{12}{16}p$, $\frac{15}{16}p$, p, $\frac{15}{16}p$, $\frac{12}{16}p$, $\frac{7}{16}p$.

Now suppose there are ties instead of braces, in the panels. Making the section at b and it will cut the acting bars cb, $b3$, and 3 2. When it is loaded from 3 to A, $c2$ will not act.

$$\text{tang}\,\theta = -\frac{l}{\text{D} - y''} \qquad . \qquad . \qquad . \qquad (16)$$

$$\text{and H} \;.\; (\text{D} - y'') = \text{V}\,(n-1)\,l$$

$$\therefore\;\; \text{H} = \frac{\text{V}\,(n-1)\,l}{\text{D} - y''} = \frac{\text{V}l\text{N}^2}{4\text{D}\,(\text{N} - n + 1)}$$

$$y'' = \frac{\text{D}}{\text{N}^2}(\text{N} - 2\,n + 2)$$

V is given in (12) and tang i in (13a); these substituted in (4) give

$$\text{F}_2 \cos\theta = \text{V}\frac{1 - \dfrac{(n-1)l}{\text{D} - y''} \;.\; \dfrac{4\,\text{D}}{\text{N}^2 l}(\text{N} - 2n + 1)}{1 + \dfrac{l}{\text{D} - y''} \;.\; \dfrac{4\,\text{D}}{\text{N}^2 l}(\text{N} - 2n + 1)}$$

$$= \text{V}\frac{n-1}{\text{N} - n} = \frac{(\text{N} - n + 1)\,(n-1)}{2\,\text{N}} \qquad . \qquad . \qquad (17)$$

This formula also fails, for $n = \text{N}$, because, in making the final reduction, we dropped a factor, $\frac{\text{N} - n}{\text{N} - n}$, which for $n = \text{N}$ becomes $\frac{0}{0}$, but it should $= 0$. It is true for all the other ties.

If in (17) we write $n + 1$ for n, we will have $(\text{N} - n)n$, which is the same as (15); hence, the vertical component of the strain on a brace, when braces are used, is the same as on the tie in the next panel, when ties are used; and as the inclinations, $c2$ and $c4$, for instance, are the same, the *actual* strains will be the same. Hence, referring to the preceding table, we have $\frac{7}{16}p$ for the vertical strain on the *second* tie (or $a2$); $\frac{12}{16}p$ on the *third* tie (or $b3$), &c.

The general principles of the methods here used are applicable to those cases in which the bays of the lower chord are not equal; but in such cases we cannot obtain as symmetrical expressions as those here found.

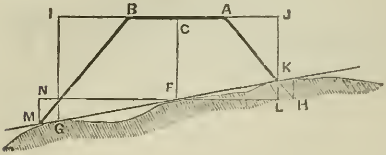
Railroad Cuttings and Embankments.—Side Depths and Side Stakes. By Oliver Byrne, C.E.

From the Lond. Civ. Eng. and Arch. Jour., Feb., 1864.

(Continued from page 152)

In an embankment (Fig. 7), given the breadth of the roadway AB = 32 feet; the height CF = 18 feet; the side slopes as 1 to $\frac{2}{3}$ (BI : ID : : 1 : $\frac{2}{3}$); the fall of the surface F to M = 1 in $26\frac{1}{2}$ (FN : NM : $26\frac{1}{2}$: 1); the rise of the surface from F to K to be the same as the fall, which is very often the case. Required the horizontal distances FN and FL, where the surface of the ground will meet the rise and slopes of the road.

Fig. 7.



$$DF = \frac{32}{2} + \frac{3}{2} \times 18 = 43 = FH.$$

$$\begin{array}{rr} 26\tfrac{1}{2} \times \tfrac{2}{3} = & 17\tfrac{2}{3} \\ \text{take} & 1 \\ \hline & 16\tfrac{2}{3} \end{array}$$

$$\frac{43}{16\frac{2}{3}} = \frac{129}{50} = 2{\cdot}58$$

43 *plus* 2·58 = 45·58 = FN.

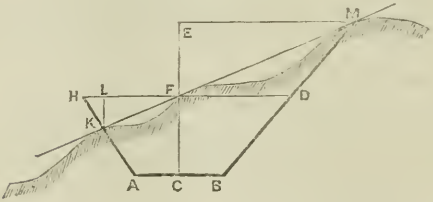
$$\frac{43}{18\frac{2}{3}} = \frac{129}{56} = 2{\cdot}3, \text{ and}$$

43 *minus* 2·3 = 40·7 = FL.

With embankments the rise answers to the fall in cuttings, by inverting Fig. 3 (*ante* page 146) it becomes Fig. 4; hence the rule given for cuttings is easily made to answer for embankments.

In a cutting, Fig. 8, given the breadth of the roadway AB = 28 feet; the height CF = 20 feet; the ratio of the side slopes 1 : ½; the inclination of the surface of the ground at the cross-section, taken by a theodolite = 14°, that is, the angle MFD = HFK = 14°. Required the horizontal distance EM and FL, where the surface of the ground meets the side slopes.

Fig. 8.



90° — 14° = 7°.

The natural tangent of 76° = 4·010781, hence the rise and fall of the slope of the surface of the ground at the cross-section may be taken as 4·01 to 1,

$$HF = \frac{28}{2} + \frac{2}{1} 20 = 54 = FD.$$

$$\begin{array}{lcr} 4{\cdot}01 \times \tfrac{1}{2} & = & 2{\cdot}005 \\ \text{Subtract} & . & 1{\cdot}000 \\ \hline & & 1{\cdot}005 \end{array}$$

$$\frac{54}{1{\cdot}005} = 53{\cdot}73$$

$$\begin{array}{lr} \text{Add} \; . & 54{\cdot}00 \text{ half-breadth.} \\ \hline EM = & 107{\cdot}73 \text{ feet.} \end{array}$$

Again, $4{\cdot}01 \times \frac{1}{2} = 2{\cdot}005$

Add . $1{\cdot}000$

$3{\cdot}000$

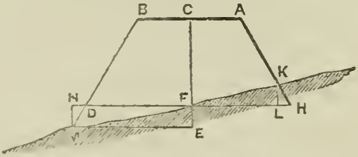
$54{\cdot}00$ half-breadth.

$\frac{54}{3{\cdot}005} = 17{\cdot}97$ take

FL $= 36{\cdot}03$ feet.

In an embankment (Fig. 9), given the breadth of the roadway AB = 30 feet; the height CF = 12 feet; the ratio of the side slopes $1 : \frac{3}{4}$; the elevation or inclination of the surface of the ground in the direction of the cross-section = 3°, that is, angle KFH = DFM = 3°. Required the horizontal distances EM, FL, where the surface of the ground meets the side slopes.

Fig. 9.



$$90° - 3° = 87°;\ \tan 87° = 19{\cdot}081137.$$

Hence the rise and fall of the surface from the centre stake F may be represented by the ratio 19·08 to 1.

$$\text{HF} = \frac{30}{2} + \frac{4}{3} \times 12 = 31 = \text{FD}.$$

$19{\cdot}08 \times \frac{3}{4} = 14{\cdot}31$

Subtract . $1{\cdot}00$

$13{\cdot}31$

$\frac{31}{13{\cdot}31} = 2{\cdot}33$

$31{\cdot}00 = \text{FD}$

$33{\cdot}33 = \text{EM}$

Again,

$19{\cdot}08 \times \frac{3}{4} = 14{\cdot}31$

Add . $1{\cdot}00$

$15{\cdot}31$

$\frac{31}{15{\cdot}31} = 2{\cdot}03$

$31{\cdot}00 = \text{FH}$

Difference $28{\cdot}97 = \text{FL}.$

The simplicity of this plan of putting down side stakes is apparent, and the formula from which it is deduced is easily remembered.

$$\text{HL} = \frac{d}{ne+1};\ d = \text{FH}.$$

$$\text{DN} = \frac{d}{nt-1};\ d = \text{FD}.$$

It should be remembered that in the ratio 1 to n, 1 represents the base and n the perpendicular of the side slopes. In the ratio t to 1, t represents the base, and 1 the perpendicular rise in the case of a cutting, and *vice versa* in the case of an embankment (see Figs. 7 and 8). The ratio e to 1, e represents the base and 1 the perpendicular.

To set out the widths when the surface of the ground is laterally sloping, and when the cross-section consists partly of cutting and partly embanking. Let K, A, QMB, Fig. 10, be the cross-section of a railroad, consisting partly of a cutting QMB, and partly of an embankment AQK. Let $t = \text{AB} = \text{DH}$, the bottom width, F the centre stump, $\text{FC} = h$ the depth of the cutting, KM the sloping surface of the ground. First, suppose the embankment AKQ to occupy less than half the bottom width, as from A to Q, Fig. 10. And further, suppose the ratio of the slope of the earth on both sides of the centre stake F, to be represented by the same ratio t to 1; that is,

$$\text{FN} : \text{NM} :: t : 1, \text{ or } \text{FL} : \text{LK} :: t : 1.$$

Yet the reasoning upon which the practical rule is founded will apply to two different slopes of ground, from the centre stake F, as in the former case. When the slope of the ground is known, or the ratio $t : 1$ is determined, then the point Q where the slope meets the base is readily found; for

$$1 : t :: \text{FC} : \text{CQ}. \quad \text{Or } \text{QC} = ht.$$

That is, if the height h multiplied by t be less than AC, half the breadth of the roadway, the work will be at the cross-section part cutting and part embankment.

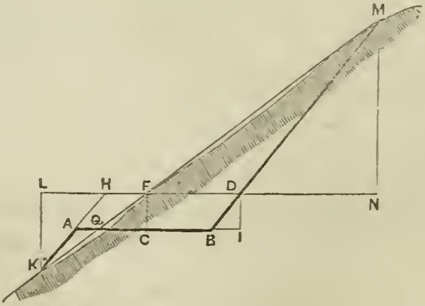
Let the side slopes be represented by the ratio $1 : n$, that is,

$$\text{BI} : \text{ID} = \text{DN} ; \text{NM} = \text{HL} : \text{LK} = 1 : n.$$

$$\text{Put } \text{DN} = x, \text{ and } \text{HL} = y.$$

The horizontal half-breadth FD, through F, to meet the slope of the cutting, is found as in the former investigation, where the slope of the ground does not meet the roadway at either side of F.

Fig. 10.



$$\text{FD} = \frac{b}{2} + \frac{1}{n} \times h, \text{ which put} = d.$$

The horizontal half-breadth FH, through F, to meet the slope of the embankment KH, is easily found when FD is known, for

$$\text{HD} = \text{AB} = b, \text{ and } \text{HF} = \text{HD} - \text{FD} = b - \left(\frac{b}{2} + \frac{h}{n}\right) = \frac{b}{2} - \frac{h}{n}$$

which put $= \partial =$ HF.

$$1 : n : : x : nx = \text{NM}.$$

$$\text{Also, } t : 1 : : d + x : \frac{d+x}{t} = \text{NM}; \ \therefore \ nx = \frac{d+x}{t}, \text{ or } x = \frac{d}{nt-1}$$

This result is exactly the same as that given before, for the increase of horizontal breadth in the case of a rise in cutting, or a fall in embanking.

$$1 : n : : y : ny = \text{KL};$$

$$\text{Again, } t : 1 : : \partial + y : \frac{\partial + y}{t} = \text{KL}; \ \therefore \ ny = \frac{\partial + y}{t}, \text{ or } y = \frac{\partial}{nt-1}$$

which is a similar expression to that given for x; the only difference is that $\partial =$ HF is put in the place of $d =$ FD.

Example.—Let AB $= b = 28$ feet; FC $= h = 4$ feet; the side slopes 1 : 2 (BI : ID : : 1 : 2): the cross slope of the ground 13 to 1 (FN : NM : : 13 : 1). Required the horizontal distances FN and FL where the slopes meet the surface.

$$\text{FD} = \frac{28}{2} + \tfrac{1}{2} \text{ of } 4 = 16 = d.$$

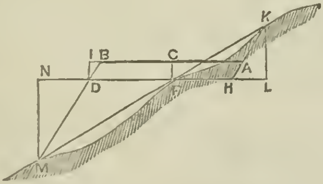
$$\text{FH} = 28 - 16 = 12 = \partial.$$

$$\text{Since } n = 2, \text{ and } t = 13, \ \therefore \ \frac{16}{2 \times 13 - 1} = \cdot 64, \text{ and } \frac{12}{2 \times 13 - 1} = \cdot 48.$$

$$\therefore \quad 16 \cdot 64 = \text{FN} \qquad 12 \cdot 48 = \text{FL}.$$

Example.—In Fig. 11, the embankment MBQ occupies more than half the bottom width, as from B to Q. Let AC $=$ CB $= 14$ feet; FC $= 4$ feet.

Fig. 11.



$$\text{FL} : \text{LK} : : 13 : 1, \qquad \text{HL} : \text{LK} : : 1 : 2;$$

as in the last example, required the horizontal distances from F to L and from F to N.

$$\text{FD} = \frac{28}{2} + \tfrac{1}{2} \times 4 = 16 = d,$$

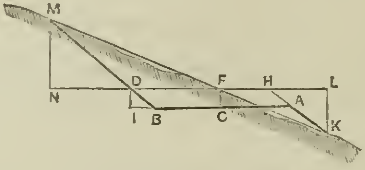
$$\text{FH} = 28 - 16 = 12 = \partial.$$

Since $n=2$, and $t=13$, $\therefore \dfrac{16}{2\times13-1}=64$, and $\dfrac{12}{2\times13-1}=\cdot48$;

Hence FN $=16\cdot64$, and FL $=12\cdot48$ feet.

Example.—In Fig. 12 the embankment AQK occupies less than half the bottom width from Q to A. Let AC $=$ CB $=14$ feet; FC $=4$ feet;

Fig. 12.



FL : LK : : 13 : 1; BI : ID : : 1 : 2.

Required the horizontal distances from F to L, and from F to N.

$$\text{FD}=\frac{28}{2}+\tfrac{1}{2}\times4=16=d.$$

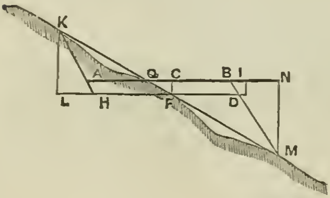
$$\text{FH}=28-16=12=\delta.$$

$$\therefore \frac{16}{2\times13-1}=\cdot64, \text{ and } \frac{12}{2\times13-1}=\cdot48,$$

FN $=16\cdot64$ and FL $=12\cdot48$ feet.

Example.—In Fig. 13, the embankment QBM occupies more than half the bottom width, from Q to B. Let AC $=$ CB $=14$ feet. FC $=4$ feet.

Fig. 13.



FL : LK : : 13 : 1;

HL : LK : : 1 : 2.

Required the horizontal distances from F to L and from F to N.

$$\text{FD}=\frac{28}{2}+\tfrac{1}{2}\times4=16=d.$$

$$\text{FH}=28-16=12=\delta.$$

$$\left.\begin{aligned}\text{DN}=x=\frac{d}{nt-1}=\cdot64\\ \text{HL}=s=\frac{\delta}{nt-1}=\cdot48\end{aligned}\right\}\text{ as in the foregoing examples.}$$

FN $=16\cdot64$ and FL $=12\cdot48$ feet.

By comparing the figures 10, 11, 12, and 13, and observing how the formulas $x=\dfrac{d}{nt-1}=$ DN, and $y=\dfrac{\delta}{nt-1}=$ HL, may be applied in each possible case, the setting out of side stakes, when part of the cross-section is a cutting and part an embankment, becomes easy.

MECHANICS, PHYSICS, AND CHEMISTRY.

For the Journal of the Franklin Institute.

On the Power requisite for the Propulsion of Steam Vessels.

By D. M. Greene, C. E., 2d Asst. Eng. U.S.N.

This problem, like that of determining the relations between the temperature, density, and volume of steam, seems never to have been satisfactorily solved: indeed, it is believed that the impracticability, if not the impossibility, of solutions based upon purely theoretical considerations, is generally conceded.

As, in the case of steam, the formulæ in use are based upon experimental results, so in the case under consideration, we must satisfy ourselves, for the present, at least, with an empirical formula, carefully deduced from the performances of steam vessels. Complete and reliable data are available, for this purpose, in the steam logs of vessels belonging to our navy.

Hitherto, it is believed to have been the custom, generally, to employ the formula given by Bourne, in his work on "The Steam Engine;" which is

$$\mathrm{HP} = \mathrm{CAV}^3; \qquad . \qquad . \qquad . \qquad (1)$$

in which

HP = the total horse power developed by the engines.
A = midship section, in square feet.
V = speed, in knots, per hour.
C = a constant for vessels of similar model;— different values of C being used for vessels of different model.

Solving (1) with respect to C, we have

$$\mathrm{C} = \frac{\mathrm{HP}}{\mathrm{AV}^3}.$$

Substituting in this expression the values of HP, A and V^3, for any vessel, the corresponding value of C, for vessels of like model, is found.

The general principle involved in the foregoing formula, may be briefly stated, thus:—*The resistance, opposed by a liquid, to the motion of a solid through it, varies as square of the velocity, and as the midship section of the body; and the work expended, in overcoming this resistance, during a unit of time, varies as the cube of the velocity, and as the section.*

Hence the power, absolutely, would be the product of the cube of the speed, into the midship section, multiplied by the constant C.

That the formula, in question, is defective, may be inferred, from the fact that the *actual* performances of steam vessels, rarely ever agree, even approximately, with the *estimated* performances.

The defect seems to be,

1st. That no account is taken of the fact, that a very considerable portion of the total power of the engines,—amounting, in marine engines, to from one-eighth to one-fifth, and varying simply as the speed and area of piston,—is expended in overcoming the back-pressure in the cylinder, together with the friction of the engine itself; and, 2d. That too much importance is attached to the differences in model, as regards fullness, or sharpness; while the great influence of *immersed surface* is not recognised.

To illustrate the force of the last objection, let us take the extreme case of a cylindrical solid, having a diameter of ten feet, its axis vertical, and drawing ten feet: the immersed surface will thus be 392·7 square feet. Next, suppose the form changed in such a manner, as to make the horizontal section of the solid take the form of two isosceles triangles joined at their bases; the draft and displacement remaining the same. Then, if the length be 31·4 feet, and the breadth (beam) 5 feet, the immersed surface will be nearly 720 square feet.

It must be clear, therefore, that while an increased length and sharpness is attended by a diminution of resistance due to itself, there is at the same time an increase of immersed surface, which acts prejudicially to the motion of the vessel; and, moreover, that when the increased resistance due to the increase of immersed surface, becomes equal to the diminution of resistance, due to increased sharpness, any further increase in length and sharpness, for a given displacement, will be attended by an absolute increase of resistance. Unfortunately, we are as yet, unable to estimate correctly, the effect of immersed surface upon the resistance, or to fix the limit to the ratio, of length to breadth, which will insure the best results: it is believed, however, that in some instances, this limit has been reached, if not exceeded.

In the construction of an empirical formula, for the purpose stated, we shall make use of the performances of vessels, some of which have lines of more than average fullness, together with one (*Niagara*) which has lines of more than ordinary fineness. It will be assumed, that *within the limits of practice*, the difference in resistance due to difference in model—when compared with the difference in resistance due to a slight variation in speed—may be neglected without considerable error. The results will show how fully we are justified in making this assumption.

In addition to the notation already used,

Let $D =$ displacement in tons.

$l =$ length in feet.

$C_1 =$ constant, depending upon the power P_1, expended in overcoming the back and friction pressures.

$C_2 =$ constant, depending upon the power P_2, usefully applied to the shaft, together with the additional friction due to that power.

Then, since P_1 varies as the speed, and assuming for convenience, that it varies also as the midship section, we have

$$P_1 = C_1 A V, \qquad . \qquad . \qquad . \qquad (2)$$

Similarly $$P_2 = C_2 A V^3. \qquad . \qquad . \qquad . \qquad (3)$$

In order to express A, in terms of D, we have, from the relations between similar surfaces and solids, and their homologous dimensions

$$A \propto l^2. \qquad \therefore \qquad l \propto A^{\frac{1}{2}}$$

Also, $$D \propto l^3. \qquad \therefore \qquad l \propto D^{\frac{1}{3}}$$

Whence $$A^{\frac{1}{2}} \propto D^{\frac{1}{3}}.$$

$$\therefore \quad A \propto D^{\frac{2}{3}}.$$

Substituting in (2) and (3), and collecting, we get

$$\begin{aligned} HP &= C_1 D^{\frac{2}{3}} V + C_2 D^{\frac{2}{3}} V^3, \\ &= D^{\frac{2}{3}} V (C_1 + C_2 V^2). \qquad . \qquad . \qquad (4) \end{aligned}$$

To find C_1 and C_2, we have from (2) and (3), after substituting $D^{\frac{2}{3}}$ for A,

$$C_1 = \frac{P_1}{D^{\frac{2}{3}} V}, \text{ and } C_2 = \frac{P_2}{D^{\frac{2}{3}} V^3}$$

respectively. Taking, now, the distribution of power, made by Chief Engineer Isherwood, U.S.N., in his works, we get in the cases of the different vessels named, the following results, viz:

NAME OF VESSEL.	C_1	C_2
Merrimack, . . .	·18184	·0047345
Wabash, ...	·14847	·0045799
Minnesota, ...	·15875	·0045725
Roanoke, ...	·13619	·0048133
Niagara, ...	·15067	.0047227
Mean values, .	·1552	·0046846

These values, substituted in (4), give, finally,

$$HP = D^{\frac{2}{3}} V(·1552 + ·0046846\, V^2). \qquad . \qquad (5)$$

The close coincidence of the constants, for the *Niagara*, with those of the other vessels, will be observed; it seems to indicate that the limit of the ratio, of length to the breadth of beam, has been exceeded; or, that the power required to propel her, is as great as it would have been, had her model been similar to that of the other vessels named.

In (5), the part, $V(·1552 + 0046846\, V^2)$ may be tabulated thus:

V.	C.	V.	C.
10	6·2366	16	21·6713
11	7·9424	17	25·6538
12	9·9574	18	30·1140
13	12·3097	19	35·0862
14	15·0273	20	40·5808
15	18·1385		

Then, in every case, we have

$$HP = D^{\frac{2}{3}} C; \qquad . \qquad . \qquad . \qquad (6)$$

C being taken from the table, to correspond with the required speed.

Example.—Required the total power of the engines which will develop a speed of 14 knots in a vessel whose displacement is 1000 tons.

From (6), $$\begin{aligned} HP &= 1000^{\frac{2}{3}} \times 15·0273 \\ &= 100 \times 15·0273 \\ &= 1502·7 \end{aligned}$$

It should be remarked, in this connexion, that the formula is claimed to apply, only in the case of the maximum performance of the vessel; for, as the speed of the vessel is diminished, the value of P_2 will diminish faster than P_1; and hence the result given by the formula will be too small. The error due to this cause, however, on ordinary speeds, will not be very great.

By the aid of the formula (5), has been prepared, the following

TABLE OF POWERS DUE TO DIFFERENT DISPLACEMENTS AND SPEEDS.

D.	V = 10	V = 11	V = 12	V = 13	V = 14	V = 15	V = 16	V = 17
1000	624	787	996	1231	1563	1814	2167	2565
1100	665	839	1061	1312	1601	1933	2309	2734
1200	704	889	1125	1390	1697	2048	2447	2897
1300	743	937	1186	1466	1790	2161	2581	3056
1400	780	985	1246	1541	1881	2270	2712	3210
1500	817	1031	1305	1613	1967	2377	2840	3362
1600	853	1077	1362	1684	2056	2181	2965	3509
1700	888	1121	1418	1756	2140	2584	3087	3654
1800	923	1165	1473	1821	2224	2684	3207	3796
1900	957	1207	1528	1888	2305	2782	3324	3935
2000	990	1249	1581	1954	2385	2879	3440	4072
2100	1023	1291	1633	2019	2464	2975	3554	4207
2200	1055	1331	1684	2082	2542	3068	3666	4340
2300	1087	1371	1735	2145	2618	3160	3776	4470
2400	1118	1411	1785	2207	2694	3251	3885	4599
2500	1149	1450	1834	2268	2768	3341	3992	4725
2600	1179	1488	1883	2328	2841	3430	4098	4851
2700	1209	1526	1931	2387	2914	3517	4202	4974
2800	1239	1563	1978	2445	2985	3603	4305	5096
2900	1268	1600	2025	2503	3056	3689	4407	5217
3000	1298	1637	2071	2560	3126	3773	4508	5336
3100	1326	1673	2117	2617	3195	3856	4607	5454
3200	1354	1709	2162	2673	3263	3939	4706	5571
3300	1382	1744	2297	2729	3331	4020	4804	5686
3400	1410	1780	2251	2783	3398	4101	4900	5801
3500	1438	1814	2295	2838	3464	4181	4996	5914
3600	1465	1849	2339	2891	3530	4260	5090	6026
3700	1492	1883	2382	2945	3595	4339	5184	6137
3800	1519	1916	2425	2998	3660	4417	5277	6247
3900	1545	1950	2467	3050	3723	4494	5370	6356
4000	1572	1983	2509	3102	3787	4571	5461	6464
4100	1598	2016	2551	3153	3850	4646	5551	6571
4200	1623	2048	2592	3204	3912	4721	5641	6678
4300	1649	2081	2633	3255	3973	4796	5730	6783
4400	1675	2113	2674	3305	4035	4870	5819	6888
4500	1700	2145	2714	3355	4096	4944	5907	6992
4600	1725	2177	2754	3404	4156	5017	5994	7095
4700	1750	2208	2794	3454	4216	5089	6080	7197
4800	1775	2239	2833	3503	4276	5161	6166	7299
4900	1799	2270	2872	3551	4335	5232	6252	7400
5000	1824	2300	2911	3599	4394	5304	6336	7501
5100	1848	2332	2950	3647	4452	5374	6420	7600
5200	1872	2362	2989	3694	4510	5444	6504	7700
5300	1896	2392	3027	3742	4568	5513	6587	7798
5400	1920	2422	3065	3789	4625	5583	6670	7896
5500	1943	2452	3102	3835	4682	5652	6752	7993

To use the Table, seek the required speed at the top, and the displacement at the side of the page; trace the columns to their intersection, and the number found will be the required power.

The vacuum, in the case of maximum performance of each of the vessels employed in determining the constant c_1 and c_2, was about 26 inches of mercury; and in the construction of the formula it has been assumed that an equal degree of rarefaction will always be secured. If the vacuum be less, in any case, then there will be an additional amount of power required, due to the difference.

As a confirmation of the close approximation to the truth of the assumptions made in the foregoing, it may be stated that, so far as it has been practicable to compare the results of practice (the actual performances of steam vessels,) with those given by the formula, the results have been invariably satisfactory.

It may be stated also, that Morin has found experimentally that, among the forms experimented upon, the form of least resistance was the *sphere:* next in order came the *cylinder*, terminated by *hemispheres*; after this came the cylinder terminated by *cones*. Bosut found that in the case of two models having angles of 12° and 24° at the bow, respectively, the resistances were sensibly equal.

Mr. Rankine, President of the Association of Engineers, of Scotland, and Professor of Mechanics and Civil Engineering, in the University of Glasgow, in his work on Applied Mechanics, p. 599, says:

"From the results of observations of the engine power required to propel vessels of different sizes and figures at different velocities, there is reason to think it probable, that when ships are built of such figures that the water glides round their surfaces without forming surge or large eddies, the principal part, if not the only appreciable part of the resistance is due to the direct friction between the water and the bottom of the ship; and if this opinion should be confirmed by further observation, it will show that the advantage of Mr. Scott Russell's 'wave line' figures, and of any other figures which may be successful in combining speed with economy of power, consists in causing the water to glide past the ship with the least possible agitation and friction; and also, that a certain definite proportion of length to breadth is best, and that not only excessive bluffness, *but excessive sharpness also*, is unfavorable to speed combined with economy of power."

Naval Academy, Newport, R. I., April 11, 1864.

Finely Divided Iron and its Application for the Precipitation of Copper from Solutions and other purposes.

From the Chemical News, No. 205.

Mr. Gustav Bischof, of Skelly, near Swansea, has built a reverberatory furnace at the works of Messrs. Roberts, Dale, and Co., Warrington, for the production of finely divided iron, by heating a mixture of pulverized oxide of iron and carbonaceous matter, avoiding fusion. The powdered iron thus obtained is used for the precipitation of copper from its solutions in the manufacture of aniline instead of iron filings, and for other purposes.

In the manufacture of aniline iron filings may be used as usual, which, having been oxidized, are re-converted into the metallic state as described, thus being employed over and over again.

For the precipitation of copper, Spanish or Irish burnt ores containing a few per cent. of copper are reduced in the above furnace. Both iron and copper in these ores are converted into the metallic state, and being employed for the process of precipitation, the latter mixes with the copper precipitated from solutions. As by these means the whole copper contained in the burnt ores is extracted, the greater value of such copper to the price paid for the ores fully covers the cost of purchase and reduction, even leaving in most cases a considerable surplus.

Trials made with the precipitation of copper from artificial solutions and mine waters by Mr. Bischof have amply proved the powerful action of such iron powder. This process therefore combines the saving of all costs for iron with a greatly facilitated and accelerated precipitation.

Compensated Pendulums for Gravity Escapements. By E. B. Denison.

From the Lond. Mechanics' Magazine, February, 1864.

Sir :—I have now had time to test by experience a discovery which I made about two years ago of a very remarkable mistake in the hitherto received calculation of the height of the mercury required to compensate a seconds pendulum of the usual construction, and especially those with a cast iron jar.

It was originally given by the late Mr. F. Bailey, P.R.Astr.Soc., in the first volume of that Society's Memoirs; and the calculation there is so long and complicated, that it is not surprising that his statement of the ultimate result has been accepted in faith by other writers. Certainly I should never have thought of wading through it if I had not found that some calculations of my own for a new pendulum to carry 28 lbs. of mercury obstinately refused to conform to Mr. Bailey's within a difference of 2 ins. in the height of the mercury.

The error is nothing less than this :—After taking into account a number of small and comparatively insignificant elements, he altogether forgot the weight of the jar itself, which is not inconsiderable, even if it is a glass one with the usual frame or stirrup; and if it is iron (which, for various reasons, is the best construction), it is never less than one-third of the weight of mercury even in a wide jar, and more in the usual 2 in. jars. And as the rise of the top of the mercury has to compensate not only for the fall of its bottom when the rod expands by heat, but for the fall of the heavy jar also, it is evident that the height of the mercury must be considerably more than if the weight of the jar might be disregarded. Exactly the same mistake was made by Captain Kater in his popular version of Bailey's calculation in Lardner's "Mechanics." In the "Rudimentary Treatise on Clocks," I gave no calculation of my own, but merely stated the generally received result, with a small addition which seemed to be suggested by experience.

For the Journal of the Franklin Institute.

On the Parabolic Construction of Ships.

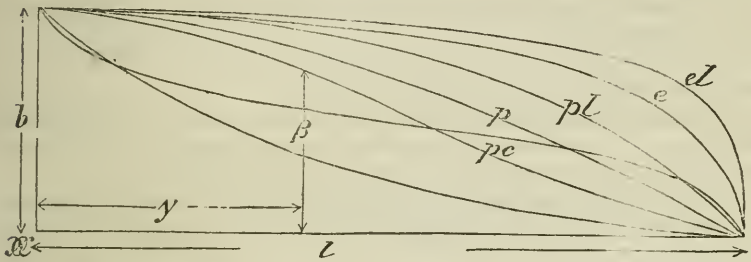
From a Lecture delivered at the Polytechnic College, Philadelphia, Feb. 11, 1864.

By John W. Nystrom, C. E.

The Parabolic Construction of Ships is now brought to perfection. Its progress has appeared in the *Journal of the Franklin Institute*, and I will here give the conclusive and fundamental formula for the system.

All the lines in a ship can be constructed by one simple formula[*], namely:

$$\beta = b\left(1-\frac{y^{n}}{l^{n}}\right)^{q}$$



$b =$ half the breadth.

$l =$ length from ⊠ to the stem or stern.

For the frames, d, or the depth from load water-line to the keel, takes the place of l.

$\beta =$ ordinate for the line.

$y =$ abscissa.

$n =$ exponent.

$q =$ power of the exponent n.

This is the general formula for the Parabolic Construction. Simple as it is, it gives any line or form of a ship that can reasonably be required. It will form a square, rectangle, triangle, circle, ellipse, parabola, hyperbola, cyma; all of any order or combination.

The variety of lines represented by the figure are obtained by altering the power q, while n remains constant; or any variety of lines can be obtained for each value of the exponent n.

It is here found necessary to the development of the subject, to propose or establish new names to such lines as have not heretofore been defined or subjected to an algebraical formula. The degree of development of an art may be correctly matured by the perfection of its vocabulary. As the construction of ships has not heretofore been brought to a perfect system, we have not been able to define the great variety of lines or forms of ships. We can say, a vessel is very sharp, or very full, with more or less rise of floor: but have no language by which to convey correctly, how sharp, how full, or with how much rise of floor. As an illustration it may be mentioned, that on one occasion I met some shipbuilders, and discussed with them the construction of ships; when one said, "I am constructing a ship that will be so sharp, that you cannot roll a barrel on the lower deck, within fifteen feet of the bow," which made me no wiser. Now, in the language of the Parabolic Construction, to convey the same idea with precision and accuracy, we have only to give the exponent and power, which not only impress the mind clearly with the correct degree of sharpness, but also with the complete form of the vessel.

We have both in Europe and America many curiously constructed vessels, and some of them reported to perform wonderfully, but we have not been able to record their peculiarities: for even the drawing of their lines would fail to convey with correctness what constitutes their novelty or folly.

It is therefore proposed to establish the following technical terms in Naval Architecture:

Any line $p\,l$, in the accompanying figure, located between the parabola p and ellipse e, to be called *paralipse.*

Any line $p\,c$, located under or within the parabola p, to be called *Paracyma.* In architecture, cymas are generally constructed of circle-arcs, but in this case cymas are derived from parabolas.

Any line $e\,l$, extending outside of the ellipse, to be called *Evolipse.*

In modern constructed vessels, those lines are generally distributed as follows:

All water-lines of the displacement are *Paracymas*, with the highest power near the keel, approaching parabolas near the load water-line, which latter may also be a *Paracyma.* The frames are generally *Paralipses* about the middle of the vessel, and terminate in *Parabolas* and *Paracymas*, in the stern and bow. Above the water, the horizontal lines are generally *Parabolas* in the foreship; and in the aftership, *Paralipses*, *Ellipses*, and *Evolipses.*

The power q defines the line as follow:

Parabola p,	$\beta = b\left(1 - \frac{y^n}{l^n}\right)^{q=1}$
Ellipse e,	$\beta = b\left(1 - \frac{y^n}{l^n}\right)^{q=\frac{1}{n}}$
Circle,	$\beta = R\left(1 - \frac{y^n}{R^n}\right)^{q=\frac{1}{n}}$
Paracyma $p\,c$,	$\beta = b\left(1 - \frac{y^n}{l^n}\right)^{q>1}$
Paralipse $p\,l$,	$\beta = b\left(1 - \frac{y^n}{l^n}\right)^{q \text{ between } 1 \text{ and } \frac{1}{n}.}$
Evolipse $e\,l$,	$\beta = b\left(1 - \frac{y^n}{l^n}\right)^{q<\frac{1}{n}.}$

In a treatise on the Parabolic Construction of Ships, now in progress, tables are calculated for eighteen values of the power q, each with 48 different exponents n, making 864 different lines; which will cover the most general requirement in practice.

The following is a sample of a part of one table:

TABLE X. PARACYMAS. $q = 1{\cdot}25$.

Exp.	Ordinates or Cross-sections.							Area or Displacement.		Cent. grav.	$\frac{2}{3}\int\beta^3 dx$
n	1	2	3	4	5	6	7	a 𝔐 D	T	e	ϕ
2	·1658	·3559	·5384	·6979	·8273	·9226	9805	·6239	·0178	·3575	·2785
2·25	·1852	3960	·5867	7444	·8643	·9450	·9884	·6512	·0186	·3660	·3025
2·5	·2071	·4340	·6302	·7841	·8936	·9610	·9930	·6735	·0193	·3735	·3263
2 75	·2288	·4700	·6695	·8177	·9165	·9724	·9959	·6953	·0199	·3893	·3445
3	·2505	·5041	·7047	·8463	·9344	·9805	·9975	·7164	·0205	·3850	·3615
3·25	·2712	·5362	·7364	·8698	·9486	·9861	·9985	·7333	·0210	·3904	·3774
3·5	·2918	·5664	·7658	·8908	·9598	9903	·9991	·7474	·0214	·3954	·3930
3·75	·3121	·5949	·7903	·9097	·9685	·9925	·9994	·7605	·0217	·4006	·4064
4	·3319	·2616	·8130	·9225	·9793	·9951	·9996	·7724	·0221	·4043	·4182
5	·4669	·7128	·8822	·9610	·9908	·9987	·9998	·8099	·0234	·4179	·4564

Either table, exponent, or power, can be employed for either frames, water-lines, or displacement.

$$\left.\begin{array}{l}\text{Area of any water-line a or } \alpha. \\ \text{“ \quad “ \quad cross-section 𝔐 or } \theta. \\ \text{Cubic content of displacement D.}\end{array}\right\} = \int \beta \, dy,$$

which integral co-efficient is contained in the column a 𝔐 D.

The depth of the centre of gravity of any cross-section, or of the displacement, or the distance from the deadflat ⊗, to the centre of gravity of the area of any water-line, or of the fore or aft part of the displacement, will be

$$e = \int \frac{\beta \, y \, dy}{\text{a, ⊗, or D}},$$

which integral co-efficient is contained in the column e.

The height of metacentre will be

$$m = \frac{2}{3\text{D}} \int \beta^3 \, dy,$$

which integral co-efficient

$$\phi = \frac{2}{3} \int \beta^3 \, dy,$$

is contained in the column ϕ.

When the power q and exponent n are given, we have,

$$\text{Height of metacentre } m = \frac{l \, b^3 \, \phi}{\text{D}}.$$

$$\text{Momentum of stability } = \text{Q} \sin. v \left(\frac{l b^3 \, \phi}{\text{D}} \pm g\right).$$

Q = weight of the vessel, and g = vertical height between the two centres of gravity.

The treatise on the Parabolic Construction, now in progress, will contain fuller explanations, with practical examples and drawings, which, on account of the tables being so complete, requires no algebraical formulas in its application.

On the Numerical Expression of the Destructive energy in the Explosion of Steam Boilers, and on its Comparison with the Destructive Energy of Gunpowder. By GEORGE BIDDELL AIRY, Astronomer Royal.

From the London Mechanics' Magazine, November, 1863.

1. A little consideration of the changes in the state of the water and steam, which occur during the bursting of a steam boiler, will show that very little of the destructive effect of an explosion is due to the steam which is contained in the steam-chamber at the moment of the explosion. The rupture of the boiler is effected by the expansive power common at the moment to the steam and the water, both at a temperature higher than the boiling-point; but as soon as steam escapes, and thereby diminishes the compressive force upon the water, a new issue of steam takes place from the water, reducing its temperature; when this escapes, and further diminishes the compressive force, another issue of steam of lower elastic force from the water takes place, again reducing its temperature; and so on; till at length the temperature of the water is reduced to the atmospheric boiling point, and the pressure of the steam (or rather the excess of steam-pressure over atmospheric pressure) is reduced to 0. It is the enormous quantity of steam, of gradually diminishing power, which is thus produced from water during the course of the explosion, that causes the disastrous effects of the explosion; compared with this quantity, the small volume of gas, which may happen to be in the steam-chamber at the time, is, in boilers of ordinary construction, wholly insignificant, and may be entirely put out of sight in the succeeding investigations.

2. If we compare the course of changes, in bursting, in two boilers, a large one and a small one, we see that the order of changes is the same in both; but that to reduce the temperature of a large body of water by a certain number of degrees, a large volume of steam must escape, whereas to reduce the temperature of a small body of water by the same number of degrees, it will suffice that a smaller volume of steam (smaller in the same proportion as the bulk of water) escape. Thus it will appear that the whole volume of escaping steam at a given pressure, and the whole destructive energy of the steam, are proportional to the bulk of water.

3. For measure of the destructive energy of the steam, we must suppose the simplest and most easily measurable case—namely, that the steam, in expanding, drives a piston along a uniform cylinder. It is necessary to ascertain the value of the pressure F when the steam has expanded so far as to have pushed the piston to the distance x: then the measure of the total energy is $\int dx.\ \mathrm{F}$, the integral being taken from the point where the piston was in contact with the water

to the point where the excess of pressure of the steam above atmospheric pressure $= 0$.

In the case of gunpowder fired in a cannon, where the weight of the ball and its velocity on emergence are found by experiment, the energy of the gunpowder as acting on the ball will be thus found:—The pressure at distance y being F', acting on a ball whose weight is W, and g being the numerical measure of the acceleration produced in one second of time by gravity ($g = 32{\cdot}1908$ if the unit of measure is the English foot, or $= 9{\cdot}8116$ if the unit is the French metre), v being the velocity at distance y, and V the whole velocity acquired, then the acceleration is $\frac{F'g}{W}$, and therefore $v\,\frac{dv}{dy} = \frac{F'g}{W}$, $v^2 = \frac{2g}{W} \int dy$. F', and $V^2 =$ total integral $\frac{2g}{W} \int dy. F' = \frac{2g}{W} \times$ total energy, whence the total energy $= \frac{W.V^2}{2g}$. And if w be the weight of the gunpowder, the energy of one unit-weight of gunpowder $= \frac{W.V^2}{2g.w}$.

5. Several years ago (before 1849) I had desired in this way to compare the destructive energy of steam from a bursting boiler with that of gunpowder; and I had requested the assistance of my friend, Professor W. H. Miller, of Cambridge (to whose knowledge of the progress of accurate science in every department of physics I have often been indebted), to enable me to give numerical values to the expressions involved. At that epoch, however, the theories and experiments on steam were not sufficiently advanced, and I was compelled to lay aside the inquiry for a time.

6. In the spring of the present year I requested Messrs. Ransomes and Sims, of Ipswich, to furnish me with an experimental result on the quantity of water escaping from a high-pressure boiler in the form of steam when the valve is gradually opened. This experiment was undertaken by George A. Biddell, Esq., Engineering Superintendent of the Orwell Works. The result was that, when the bulk 22 cubic feet of water in a locomotive boiler was raised to the temperature which produed a pressure of 60 lbs. per square inch, and when after raking out the fire the valve was gradually opened without every precaution against priming, the quantity of water which escaped in the form of steam was $2\frac{3}{4}$ cubic feet, or one-eighth of the whole.

7. Possessed of this experimental fact, I again referred to Professor Miller for such theories and citations of experiments as might be required. And by his kind assistance I was enabled to complete the investigation. And here I may state that the whole which follows is Professor Miller's with the exception of the integration of the steam-pressures, the inference from the cannon-experiments, and the comparison of steam and gunpowder.

8. In giving the heads of Professor Miller's theory, I must premise that the temparature are Centigrade, the unit of linear measures is the metre, and the unit of weight is the kilogramme. The formula adopted

as connecting the volume of steam with the volume of water (at maximum density) from which it was generated, is Fairbairn's and Tate's (Phil. Trans. 1860, p. 219). The formula for the number of *calories* required to convert water into saturated steam of temperature T, and the pressure at temperature T, are from Regnault (*Memoirs de l' Institut*, vol. xxi., pp. 748 and 728).

9. The first part of Professor Miller's investigation applies to Mr. Biddell's experiment. The steam-pressure of 60 lbs. per square inch is represented by a column of mercury (at 0 deg.) 3·1028 metres in height. Adding the atmospheric pressure 0·76 metres, the entire elastic force of the steam is represented by a column of mercury 3·8628 metres in height. The corresponding temperature of saturated steam by Regnault's Table, is 152·84 degrees. Now the quantity of water is 22 cubic feet, which at 100 degs. weighs 597·1 kilogs., and the heat requisite to raise the temperature of this water from 0 deg. to 152·84 degs. is $597·1 \times 154·38$ *calories* (the last number being derived from Regnault's formula $T + 0·00002\ T^2 + 0·0000003\ T^2$, where T for this instance $= 152·84$), or 92,182 *calories*. When all the steam has been blown off, the 597·1 kilogs. of water are separated into x kilogs. of steam at 100 degs., and $(597·1 - x)$ kilogs. of water at 100 degs. [This applies strictly when the steam has blown into a cylinder and has driven a piston, because then there may be such intercommunication of temperature between the portions of steam as will ensure that the final state of the steam is that of saturated steam at 100 degs.; it is probably true or very approximate when the steam has blown out at different temperatures and has been lost in the atmosphere.] To heat $(597·1 - x)$ kilogs. of water from 0 deg. to 100 degs. requires $(597·1 - x) \times 100·5$ *calories;* and to convert x kilogs. of water at 0 deg. into steam of 100 degs. requires $(606·5 \times 0·305 \times 100) \times x$ *calories* (by a formula of Regnault's). Supposing, then, that the amount of heat as measured by the number of *calories* is not altered by the blowing out from the boiler,

$$92{,}182 = (597·1 - x) \times 100·5 + 637 \times x,$$

whence x, the weight blown out as steam, $= 59·8$. This, however, is equivalent to only 2·2 cubic feet of water, instead of 2·75, the quantity which Mr. Biddell found to have passed away in steam.

10. Professor Miller supposes the difference to be caused principally by the heat of the mass of iron which surrounds the water; any burning fuel which may have been left in the fire-box would add slightly to its effects. It appears best, therefore, to assume the experimental fact, and to infer from it what quantity of heated water we ought to add (in investigation) to the quantity of water really present in the boiler, in order to produce correctly the amount of water which in the experiment was blown out as steam. Now 2·75 cubic feet of water at 100 degs. weighs 74·638 kilogs. Let y represent the number of kilogs. in the bulk of water which may be considered equivalent to the compound consisting of 22 cubic feet of water, the unknown weight of iron, and the unknown quantity of fuel. To heat y kilogs. of water from 0 deg. to 152·84 degs. requires $145·38 \times y$ *calories;* and this is the

amount of heat in the complex equivalent before blowing off. To heat $(y-74{\cdot}638)$ kilogs. of water from 0 deg. to 100 degs. requires $(y-74{\cdot}638)\times 100{\cdot}5$ *calories;* and to convert 74·638 kilogs. of water at 0 deg. into steam at 100 degs. requires $74{\cdot}638\times 637$ *calories;* and the aggregate of this with the last, or $(y-74{\cdot}638)\times 100{\cdot}5+74{\cdot}638\times 637$, represents the number of *calories* in the complex equivalent after the blowing off. Making this equal to the number before blowing off. $154{\cdot}38\times=(y-74{\cdot}638)\times 100{\cdot}5+74{\cdot}638\times 637$, whence $y=743{\cdot}2$.

Comparing this with the weight of the 22 cubic feet of water alone, or 597·1 kilogs., it appears that the heated materials extraneous to the water produce the effect of 146·1 kilogs. of water.

11. Assuming then that there are really 743·2 kilogs. of heated water, the investigation of the destructive energy proceeds thus:—To heat 743·2 kilogs. from 0 deg. to 152·84 degs. requires $743{\cdot}2\times 154{\cdot}38$ *calories* $=114{,}740$ *calories ;* and this is the quantity of heat for which we must account in every stage of the expansion, when the steam is allowed to blow into a cylinder and drive a piston before it. Now at any instant let w be the number of kilogs. of water converted into saturated steam; T the common temperature of water and steam; Q_T the number of *calories* required to heat 1 kilog. of water from 0 degree to T; Λ_T the number of *calories* required to convert 1 kilog. of water at 0 degree into steam at T; P_T the pressure of saturated steam at T in milimetres of mercury at 0 degree; K_T the same pressure in kilogrammes per square decimetre (all which are given for numerical values of T by Regnault); V_T the ratio of the volume of saturated steam under pressure, P_T to the volume of the water at 0 deg. from which that steam is derived (which is given by Fairbairn's formula). Then, forming the expressions for the number of kilogs. of water and steam respectively, and multiplying each by its corresponding number of *calories*, and equating the aggregate to the original number of *calories*,

$$114{,}740=(743{\cdot}2-w)\times Q_T+w\times\Lambda_T.$$

From this formula, with any assigned numerical value of T, w (the number of kilogrammes of water converted into steam) is found in numbers. And V_T the ratio of the volume of the steam generated to that of the water from which it is generated, is taken in numbers from Fairbairn's formula. And a kilogramme of water occupies one cubic decimetre of volume. Therefore the volume of steam, in cubic decimetres, is $w\times V_T$, of which w are left in the boiler to occupy the place of the expanded water; add the volume of steam expelled from the boiler is $w\times(V_T-1)$ in cubic decimetres, or $\frac{w}{1000}\times(V_T-1)$ in cubic metres.

12. Suppose, now, that the steam in escaping enters a cylinder whose section is 1 square metre, driving a piston before it. Let z be the distance to which the piston has traveled (the unit being the metre). Then $z=\frac{w}{1000}\times(V_T-1)$. And the pressure of the steam on the piston (the unit being the kilogramme) is $100\times K_T$. Therefore the two ele-

ments, the distance of the piston and the pressure upon it, can be calculated numerically for any number of numerical values of T. To find the effective pressure, the pressure first found must be diminished by the atmospheric pressure, or by the pressure of steam at 100 degrees, and it thus becomes $100\times(K_T - K_{100})$. The limit of the length of the cylinder will be determined by finding where the steam pressure = atmospheric pressure. By Fairbairn's formula, 74·638 kilogrammes of saturated steam at 100 degs. (the quantity which escaped in Mr. Biddell's experiment) occupy 122·28 cubic metres; of this, 0 0746 cubic metre remains in the boiler, taking the place of the water from which it was produced; the whole volume expelled is therefore 122·21 cubic metres, and the limiting length of the cylinder is 122·21 linear metres.

13. By these methods Professor Miller calculated the following corresponding values of *z*, the distance to which the piston has traveled (the unit being the metre), and F the effective pressure on the piston (the unit being the kilogramme). The degrees of temperature are also given, as they are the elements from which *z* and F are computed; but they are not in any way used in the subsequent calculations.

T. Degrees of Centigrade.	*z*. Metres.	F. Kilogrammes.
152·84	0	42.185
150	1·621	38,356
145	4·743	32.162
140	9·345	26.615
135	14·740	21.668
130	21·541	17.271
125	30·136	13,377
120	40·976	9,943
115	54·703	6.926
110	72·186	4,288
105	94 325	1,991
100	122·21	0

14. The effective energy of the expanding steam, as shown (for instance) by the momentum communicated to a material piston, will be represented by the integral $\int dz \, . \, F$. As the symbolical form of the function F is not known, it is necessary to perform the integration by quadrature. For this purpose I laid down the twelve data of this Table graphically (taking *z* as the abscissa, and F as the ordinate), and drew a curve by hand through the points so defined. Then I measured the ordinate for each of the values of *z*; 0, 1, 2, 3, &c. And I integeated them by the formula $\frac{1}{2}$ (first ordinate+last ordinate)— sum of intermediate ordinates—1·12th sum of second differences; where it is seen that sum of second differences is sensibly equal to—first of first differences. Thus I found the integral

1,131,400

which is the true measure of the energy of the 22 cubic feet of water at the temperature which produces the pressure 60 lbs. to the square inch, in a hot iron boiler, the units of the energy being the metre and the kilogramme.

15. If this be diminished in the ratio of 743·2 kilogrammes (the

fictitious weight of water on which Professor Miller's calculations are made) to 597·1 kilogrammes (the real weight of water), then we shall have for the measure of the energy of the 22 cubic feet of water at the same temperature unassisted by the hot iron of the boiler,

909,000;

and if we divide the two numbers by 22, we have for the energy of one cubic foot of water a temperature producing a pressure of 60 ℔s. to the square inch.

As surrounded by hot iron, 51,400. (This is the practical value.)

Without influence from surrounding iron, 41,300. (This is the philosophical value.)

16. I now proceed with the evaluation of the energy of gunpowder. The formula applicable to cannon experiments is given in Art. 4. Professor Miller referred me to a series of experiments by General Didion, in his *Traite de Balistique*, p. 485. These experiments were made with cannon of four different bores, and with eighteen different charges of powder, in each cannon. The first thing to be done was, to find by trial for each cannon, by means of the formula $\frac{W \cdot V_2}{2g \cdot w}$ (which for a single cannon may be reduced to $\frac{V^2}{w}$), what was the charge of powder in which the momentum produced bore the greatest proportion to the weight of the powder. It was found to be the following: —

With ball of	12 kilogs.,	1·500 kilogs. of powder.
"	8·07 "	1·250 "
"	6·08 "	0·875 "
"	4·05 "	0·625 "

Then adopting these four for comparison among themselves, by the formula $\frac{W \cdot V^2}{w}$, it was found that the cannon in which the powder was most efficient was that with a ball of 6·08 kilogrammes. Here it may be desirable to state that the bore was 0·1213 metre, the diameter of the ball 0·1182 metre, the length of the bore 2·815 metres; and, with the charge of powder 0·875 kilogramme, the velocity of the issuing ball was 400 metres per second. Applying the formula $\frac{W \times V_2}{19{\cdot}623 \times w}$, the energy of 1 kilogramme of gunpowder (as fired in a cannon) is found to be

56,656,

and that of an English pound of gunpowder.

25,700,

the units being in all cases the metre and the kilogramme.

17. Comparing this with the numbers found in Art. 14, we have,

The destructive energy of 1 cubic foot of water at the temperature which produces the pressure 60 lbs. to the square inch, surrounded by hot iron, is precisely equal to the destructive energy of 2 lbs. of gunpowder as fired in a cannon.

18. The destructive energy of the hot water, however, abstracting the effect of the surrounding hot iron, is considerably less than the number used in this comparison; and the destructive energy of the gunpowder, abstracting the effects of windage, cold iron, and short barrel, is considerably greater than the number used for it. Without pretending to form an accurate estimate of these effects, I think that their combination, with that affecting the energy of the water, may have diminished the apparent proportion of the energy of gunpowder by one-half. In that case,

The destructive energy of 1 cubic foot of water at the temperature which produces the pressure of 60 lbs. to the square inch, is equal to that of 1 lb. of gunpowder.

On the Duty of the Cornish Pumping Engines. By Mr. W. Morshead, Jr.

From the London Artizan, Dec., 1863.

It appeared from a tabular statement prepared by the proprietor of "Lean's Engine Reporter," for the years 1841 to 1860 inclusive, that the average duty of these engines had fallen off from 68 millions in 1844 to 52 millions in 1860, or 25 per cent.; also that less interest was now felt in the performance of these engines, as while fifty were reported in 1841, only fifteen were reported in 1858 and twenty-five in 1860. Although the nominal, or reported duty showed this marked diminution, it was not asserted that there had been an actual falling off to the extent thus indicated;—for the duty paper did not take into account the quality of the coal, which was certainly inferior to that used twenty years ago; besides which the present practice of sinking the engine shaft, for the whole, or part of its depth, in an inclined direction upon the course of the lode, must have tended to increase the friction of the pit work, and the mines were also deeper than formerly. Nor was expansion of steam adopted to so great an extent now as it was some years ago; it was then carried further than was compatible with safety, as was evinced by the repeated breakages of the main rod, the piston rod, and the other principle parts of the engine. But after allowing for all these legitimate causes of the falling off of duty, it was thought that the average duty of the county was still at least ten millions below what it should be.

The Author next examined the causes of this decline, and then discussed the means by which it might be remedied. The primary cause he believed to be, the indifference of the mine proprietors to the performance of the engines. So many accidents attended the use of high steam, cut off at an early part of the stroke that economy of fuel came to be regarded as synonymous with repeated breakages; but it was quite possible to raise the duty considerably above the present average, without resorting to an undue rate of expansion. This might be accomplished by a more perfect and extended system of reporting the engines, and by a new form of duty paper, embracing the following additional items:—First, that the load upon the piston should be taken from an indicator diagram, and from the load thus ascertained the duty should be computed, the difference between the load upon the piston and the weight of water actually lifted, that was the loss by the friction of the pitwork, &c., being placed in a separate column. Secondly, that the part of the stroke at which the steam was cut off, as well as the vacuum obtained, should be stated opposite each engine. Thirdly, that a notice of the quality of the coal used, as far as it could be ascertained, should be added. And lastly, that the engines should be separated into two classes, those which might reasonably be expected to give a good duty, and those which, from the time they had been at work, their small size, or other causes, could not fairly compete with the former. By taking the load upon the piston from an indicator diagram, a fair estimate of the work actually done by the engine could be formed, while by placing the differance between the load upon the piston and the weight of water actually lifted in a separate column, encouragement was offered for improvement in the construction and fixing of the pitwork.

At present only about one-tenth of the engines at work in Devon and Cornwall appeared in the monthly reports. If mine proprietors would co-operate in supporting a good form of duty paper, there was little doubt but that there would be a rapid and marked improvement in the duty of the Cornish engines.

Photographing Natural Colors.

From the Practical Mechanic's Journal, December, 1863.

It has often been asserted, but never yet substantiated, that the photographic process was capable, under certain mysterious processes, of reproducing the colors of the spectrum, and not merely the gradations of black to which we are accustomed. A few years ago, an American, a Mr. Hunt, startled the scientific world—at least that part of the world which knew anything about photography—by insisting that he could produce any color in the photograhic image. Practically speaking, this announcement came to nothing. Now, we find in Milan, something at least has been recently done for the furtherance of the idea. Suppose it be required to color the photograph of a man in a black coat, whose hair and beard are fair, and whose figure is projected on a white foreground, slightly shaded off, the process of the inventors is as follows:—The photograph, taken by daylight, lies in a basin full of water: it is dark, and the subsequent operations are performed by candle-light. Two solutions are at hand, one A, consisting of one gramme of chloride of gold and ten of acetate of soda, dissolved in 1000 grammes of water; the other, B, consisting of 20 grammes of hyposulphite of soda dissolved in 100 grammes of water. There are besides two more basins with water, and a quire of blotting paper. The photograph is taken out of the water and put between the leaves of blotting paper; it is then laid flat on a pane of glass, and the whole surface, except the face and hands, receives with a water-color brush a coating of solution B. By this means the parts subjected to the action of the gold soon change their tints into black. The photograph is then put into clean water again, and left there for a few minutes, during which the operator prepares a second photograph if required. The former one being taken out, is put into solution B, where it stays for a few minutes, and is then washed and rinsed as usual. Now, as the time of immersion will influence the depth of color, by successive immersions an orange-colored cravat will be obtained in one minute, a coffee-colored great-coat in five, violet-colored trousers in ten, and a black coat in thirty minutes, while the hyposulphite of soda, or solution B, gives color to the flesh and hair. Hence certain colors, though not quite the natural ones, may be obtained, which is a decided step in advance of what had been previously achieved.

Theory of Magnetic Storms.

From the London Athenæum, Jan., 1864.

In a paper read at the last meeting of the Royal Society, the Astronomer Royal propounds a theory to account for the phenomena known as magnetic storms. The behavior of water under the influence of different currents and various obstacles, as, for example, among islands, has been often observed and described; and this may be taken as one illustration. Another is found in the behavior of air during disturbances of the atmosphere. These phenomena, in Mr. Airy's opinion, represent the movements of a magnetic ether which he supposes to overspread the whole surface of the earth as an impalpable fluid envelope several feet in thickness. If we then conceive this magnetic ether "to be subject to occasional currents produced by some action or cessation of action of the sun, which currents are liable to interruptions or perversions of the same kind as those in air and water," we have a theory by which the disturbances that occur in the observed phenomena of terrestrial magnetism may be explained. This theory, emanating from so important an authority, will, no doubt, attract attention in many quarters. Mr. Airy believes that the questions involved might be decided by a series of observations made "at five or six observatories spread over a space less than the Continent of Europe." For accuracy of results he would prefer self-registering apparatus properly constructed.

"The report of my communication to the Royal Society, 'On Magnetic Storms,' contained in the *Athenæum* of the 2d of January (p. 23, col. 3), is perfectly correct except in the mention of the supposed depth of the magnetic ether, which is stated in the report of the *Athenæum* as perhaps several feet in thickness. In my communication to the Royal Society I made no allusion to the probable measure of the magnetic ether's depth; but I may now state, as my idea, that the supposed ether extends much higher, perhaps as high as mountain-tops, possibly as high as the atmosphere; and somewhat lower, probably permeating the external crust of the earth. These suppositions do not depart from the character of 'surface-current,' a term which I used in the discussion at the Royal Society's meeting.

"Yours, &c. G. B. AIRY."

Royal Observatory, Greenwich, Jan. 4, 1864.

For the Journal of the Franklin Institute.

The Proposed Dictionary of Mechanical Fallacies.

As this has been thought rather anomalous and outré, a few more thoughts on it may not be deemed out of place. The proposition is not so preposterous as it may seem, nor is it so at all. The principle is universally recognised. In all things we need to be shown the evil and the good. To appreciate the one we *must* know something of the other. While ignorant of follies how can we become wise? Intelligence is a ceaseless struggle with error in social, civil, political, philosophical, mechanical, and every other department of life. In none is truth obvious at first; so far from lying on the surface to be stumbled on, it has to be sought for, dug for, and that earnestly and perseveringly. There are what may be called ordinary deductions, extrinsic rather than intrinsic, but truth is the inbeing, the essence of excellence in every thing. In the arts it is the shortest way to an object or end; it goes to it in straight lines, while error stumbles zigzag around it. It is swiftness, strength, elegance, or whatever the attribute may be. In forms and proportions it is beauty; in the absence of meretricious accessories and ornaments, it is taste. The simpler a machine is, the truer it is, for the expression of truth is simplicity. Then mechanical like moral verities only are enduring.

Another point about truth is, that people never speak of it as something to be estimated, as are lands, money, and goods, *by the quantity.* The idea seems to prevail that where it is, error cannot be, that the two can no more mingle than fire and water; whereas, truth is in reality alloyed in every conceivable proportion. Here it is largely manifest; there, like a grain of silver in a pig of lead, its presence is not suspected. It is therefore something of which a little will not suffice in physics any more than in morals. It may exist in a mechanical process or device, and be neutralized by association with fallacies ; of which every poor machine is a proof. Results can only improve as it predominates; and herein lies the difference between the works of nature and those of man. In the former all is truth and consequently harmony and beauty, while discords and deformities characterize the latter. The best of these are to be improved by adding to the amount of truth in them.

As a class, to none is the caution, "Beware of Fallacies," more necessary than to inventors. Of all men they ought to know that truth is only to be reached through conflicts with error—through that which steals on them in flattering guise and captivates them with plausible counterfeits. It may seem strange that so many keen and shrewd inquirers fall into mechanical sophistries, and what is worse, fall in love with them, but the illusions are too seductive to be resisted. The fact is as true in physics as in morals,

> "When fiction rises pleasing to the eye,
> Men will believe because they love the lie."

Witness those who yet befool themselves with Perpetual-Motions, the grossest of mechanical falsehoods.

In ordinary as in the highest affairs, directions abound to prevent people from going astray. We have guide-posts for travelers on common roads, and Government spends millions to make seamen acquainted with shoals and other dangers of navigation by day, and on beacons and lighthouses, to direct them into safe harbors at night. For the benefit of merchants there are in most cities, "Commercial Agencies," institutions whose only capital consists of dictionaries of solvent and insolvent traders. And have we not lists revised weekly of counterfeit bank bills, and spurious coin? Naturalists are as earnest in discovering and making known noxious, as innocuous animals and plants, while chemists keep adding to the list of vegetable and mineral poisons. Such are examples of measures adopted to make evils known that they may be avoided, and which are increased and extended as occasions call for them.

Why then, it may be asked, has nothing been done for those who are devoted to the progress of the arts to warn them of paths that lead them astray? Why should they be left without compass or chart to find their way over the ocean of speculation? Simply because the necessity for such help was not perceived or not sufficiently felt. If not yet palpable, it cannot, we should suppose, be much longer ignored. There is no valid objection to its removal, for whatever may be thought of the exposure of moral offences, the making public transgressions of mechanical laws can do no harm, but much good. A hand-book of errors is a guide book to truths. It would open the eyes and diminish the number of offenders.

As progress depends upon original thinkers, and especially those whose labors comprise the magnificent and minutest exemplifications of applied science, a work calculated to save them from loss of time, and much useless expenditure of thought and labor, must be of permanent and universal interest. But as intimated in a previous notice, its value would not be confined to them. The reform it would work out in the Patent Office should not be overlooked, in abridging its labors, putting an end in a great measure to inadmissible claims, to disappointments and angry feelings attending rejected applications, to appeals of members of Congress in behalf of their constituents, to contingent fees of agents, and to other improper influences and interferences. Added to which it would raise the value of every legitimate claim, diminish the number of lawsuits, and give to the office an honorable reputation as an Institution of Science. Its reports would cease to record monopolies for ancient contrivances and modern abortions.

The running of anything like a parallel between the laws or princiciples of ethics and physics may be unusual, but is not improper, since they are intimately and inseparably allied. Emanations of the divine mind, they are equally divine. The same in their origin, essence, and purpose, it is they that impress the Deity on the material and the immaterial; hence the visible creation is an exhaustless exposition of the principles on which the arts must for ever rest; and nothing is more certain, whatever may be the case with transmundane intelligences, than that our species can only advance in civilization and happiness, as it advances in the knowledge and application of them. With us, moral truths begin with, and keep pace with, the physical. As a savage, man has no morals: they are incomprehensible to him. "Even the Arab robs the traveler with as little compunction as a bald eagle wrongs a fish-hawk, while the Fejiian eats a missionary as unconscious of guilt as a tiger feasting on an antelope, or a lion making a meal of a Bojesman." Material are moral forces. The hut and the hearth, the spindle and loom: the plough, forge and lathe; steamships, railroads, and printing presses, are products of mental and contributions to moral power. I mention these things because, to my mind, they are prophetic of the exaltation and influence of mechanical science to a degree that has probably not yet been suspected; and that the higher it rises the closer will be found its affinity with moral science. This *must* be so, since Truth in its infinitely varied forms and relations, like its Author, is ONE.

There are, and perhaps always will be, those who suppose the climax of mundane felicity was reached long ago. Panegyrists of former times, they think the world grows worse as it grows older. Looking back to a legendary age of gold, one of imaginary innocence and happiness, they mourn that it has passed away. An opposite and healthier sentiment now prevails, viz: that the elements of civilization were never so numerous and so efficiently active; that great as has been the modern increase and diffusion of knowledge, and wonderful the achievements in science and the arts, all are but preliminary to others still greater. This is a corollary of the prime doctrine of progress; that nothing in creation remains stationary or is designed to retrograde. While we realize all that we are prepared for, we transmit it with an increased volume and impetus to our successors, whose mission it will be to forward it, in like manner, to theirs; every age being a debtor to the preceding one, and a creditor to that which follows it. Is it asked, how long can this continue—Can additions to human knowledge be perpetual? Yes: at all events, as long as the forms, motions, and properties of matter here are not worn out. But may not the intellect become unequal to the successive development of new principles and management of new forces? No: It is indisputable that the more it is expanded, the more readily it apprehends and lays hold of new truths; and it is in a high degree probable that these become more lucid and simple as they become more comprehensive and sublime. E.

NEW YORK, February 20, 1864.

On the Influence of Megass on Animal Decomposition. By HENRY G. DALTON, M.D., Georgetown, Demerara.

From the Journ. Soc. of Arts, No. 580.

I have noticed some particulars attending animal decomposition when subjected to the influence of megass, or the dried stem of the sugar cane after the saccharine juice has been crushed out by machinery. But before entering upon the circumstances which first led me to consider this subject, it may be as well to state that the sugar cane, which is sufficiently known to render any description of it quite unnecessary, after having been crushed by massive iron rollers worked by steam power, is stacked under large sheds open at the sides, but protected by slated or wooden roofs, where it is kept until sufficiently dried to be used as fuel for the boilers. When first stacked, it is known as green megass, being then in a more or less fresh state, and emits a powerful sweetish odor, and is evidently the resort of millions of small gnats and other minute insects, for towards morning and evening the Megass logies, as they are called, are surrounded by swarms of swallows and other birds which evidently come here to obtain a bountiful repast.

But in this raw or green state there exists another striking and peculiar condition; there is an enormous amount of heat developed, causing the hands if thrust in to be quickly withdrawn, and a certain amount of steam or vapor is noticed. Both in this state and when more completely dessicated, megass is very combustible, and fires, spontaneous or accidental, frequently occur in these logies. It has often occurred to me to think that certain gases are generated during these changes, which probably tend to explain some of the peculiar facts about to be noticed.

On the 31st January, 1863, I was summened to attend a coroner's inquest, and examine a dead body found in one of the megass logies on an estate on the west bank of the river Demerara. On approaching the spot where the corpse was stated to be, there was no fœtid odor as usual to indicate its presence, and on climbing up the dried megass, I saw the corpse of a human being partially embedded in megass, but without odor, and in a completely mummified condition. There was a fold of cloth twisted round the neck, and this joined another fold attached to a beam close by, leading one to suppose that death had occurred from strangulation. The body was completely dessicated, shrivelled and brittle, so much so that on tryiug to draw the corpse towards me by means of a pick for the purpose of examination, I was surprised to find the head separate completely from the trunk. On more closely examining the body the following facts were noticed:

There was not the usual odor of a body long since lifeless, and exposed to the action of the air in a tropical climate.

The muscles, soft parts, and internal organs were shrivelled, dry, and parchment-like in character.

The sex (male) barely distinguishable; the lower limbs were bent at an obtuse angle, and the skin and muscles clung round the bones like paper stretched over boards; the skull and face were denuded of all muscular and ligamentous traces, so that the disarticulation of the lower maxilla and cervical vertebræ was complete.

By the shaven crown and tuft of long hair, I recognised the body to be Chinese, and the only identity as to name, &c., arose from the peculiarities of the teeth, and from the absence of certain of them, which enabled some of the other Chinese immigrants to conclude that the deceased was one of their countrymen, who had been missing from the estates for several weeks.

This body, then, had been lying loosely covered with megass in the place where it was found for probably several weeks, during which time, although laborers were constantly in the neighborhood, it had attracted no notice by any noxious effluvia, nor was it even spied out or scented by the carrion vultures (*Cathartes Jota*), who seldom fail to detect dead carcases of any description, however obscured in the mud or the tangled bush of this colony.

Struck with so singular an appearance and condition, and with the remarkable absence of noxious smell, it seemed to me to be the result of some influence exercised by the megass on which the body had fallen or been thrown, and with a view to elucidate this, I instituted the following experiments:

1st. On the 1st April, 1863, I buried a dead goat loosely in megass, and visited the spot on the 3d, but could only detect a faint odor of decomposition, quite unlike the intolerable stench which is so common here in dead bodies exposed to the sun and air. On repeating my visits a few days afterwards, I found that the dead goat had disappeared, and on inquiry, found it had been stolen by some Chinese, who had speedily devoured it, so that it could not have been very offensive, for although our celestial immigrants are incorrigible thieves, and not very particular as to the quality and condition of their food, they would hardly have appropriated this first subject of my experiment had it not been more or less preserved by the agency of megass.

On the 27th of the same month I buried another dead goat in a similar manner, but somewhat more securely out of reach of the Chinese, and disinterred it about 30 days after, or on the 27th May. During that time I had repeatedly visited the neighborhood, accompanied by others, and not one of us could detect the slightest offensive odor. On examining the body, which by-the-bye was completely disintegrated, there was no other trace of noxious effluvia than is peculiar to the dried skin of an animal. The head was denuded of all muscular and ligamentous attachments. The orbits were empty, and the bones forming it perfectly clean, likewise the nasal structure, palate, and jaw-bones. Several disarticulated vertebræ were found loose and totally void of muscles and ligaments, likewise the ribs and other parts of the body, as far as they could be gathered; the outer casing of one horn was completely detached from its osseous support, and the rest of the carcase was in a fragmentary state, dry and scentless. There were no ants or other insects visible to account for this complete disintegration, but possibly whilst the noxious odor of decomposition was neutralized by the heat or gases developed by the megass, the work of destruction of the soft parts might have been carried on during the month of interment by ants and other insects, although no trace of them was visible at the time of my inspection.

Of the many practical advantages to which the discovery of the disinfecting properties of megass, if confirmed by further experience, might apply, I do not think I need offer any remarks at present, or until the subject has been more fully examined. I may mention, however, that an occasion lately presented itself by which I was enabled to test its value, and to prove its efficacy as a very useful agent in a sanitary point of view.

Shortly after my experiments on the properties of the megass, I was much concerned at the outbreak of a number of cases of sloughing ulcers on the feet and legs of the East Indian immigrants (Coolies and Chinese), in one of the Estates' Hospitals under my medical charge, and my anxiety was still further increased on the appearance of hospital gangrene among some of the worst cases. Not only in the wards where the unfortunate patients slept, but throughout the whole hospital, the effluvium was most offensive and disagreeable. After making use of several medical disinfectants in vain, I determined to make a trial of the megass.

I desired several large baskets to be filled with it, and placed them in the corners of the wards by the doorways, and in the gallery to windward of the rooms, and on my next visit was agreeably surprised to find the obnoxious odors greatly lessened. I continued its use, and in a short time (two or three days) the change for the better was very remarkable. Except in the immediate neighborhood of the worst cases, there was no unpleasant odor, and even then the disagreeable smell was greatly modified.

Unfortunately there was no corresponding improvement in the character of the sores decidedly attacked by gangrene, but the general improvement in the hospital was manifest, for not only was the unhealthy effluvium neutralized by the megass vapor, but the peculiar, pleasant, and sweet flavor of the cane, was disseminated through the whole hospital, rendering its atmosphere fresh and agreeable, so much so, that in spite of the disappearance of the gangrene, and the existence merely of ulcers of an ordinary kind, its use and benefit are still appreciated by the nurses and patients, who are assiduous in constantly keeping up a fresh supply.

In submitting these few remarks on a subject altogether new, I think I may venture to state that in green megass we possess an agent competent materially, if not entirely, to neutralize noxious gases, and thus by arresting or destroying the offensive odors of animal decomposition, and the foul air of hospitals, to prove of eminent service and benefit in a tropical country, where these unpleasant and injurious conditions are too apt to prevail.

On the part that Titanium plays in Pig Iron and in Steel.

From the London Practical Mechanic's Journal, December, 1863.

A valuable paper was read at the Newcastle meeting of the British Association by Mr. Edward Riley, F.C.S., "On the occurrence of Titanium in Pig Iron, and on the use of Titaniferous Minerals in the manufacture of Iron and Steel." The coppery red cubic crystals examined long ago by Wollaston, and procured from the hearths of the South Wales Blast furnaces, were considered by him as metallic Titanium, and passed as such for years.

Wohler subsequently proved that they were a mixture of azoturet and cyanide of titanium, and contained 18 per cent. of nitrogen and 4 per cent. of carbon.

It is quite clear, however, that the history of this body is not yet complete, for we had long since ourselves remarked what Mr. Riley mentions, that in a mass of these crystals, as taken from the hearth, some are attacked by nitric acid and converted into titanic acid, while others are perfectly insoluble in that menstruum.

Mr. R. Mushet, under whose hand metallurgic patents grow month by month like mushrooms, has taken abundant care to advertise the world of the existence and value of his "Titanium Steel," and other similar metallurgic marvels. The proverbial "Dowsterswivel" reputation that mercantile metallurgy of the thaumaturgic and recondite sort has but too justly acquired, clung with many pretty well-informed persons to the whole titanic brood—when they called to mind that no one had been able chemically to detect any titanium in even Mr. Mushet's own titanium steel; that Mr. Riley in confirmation of this says, he was never able to find it in more than uncertain traces, and that Dr. Percy, and St. Claire De Ville, who had paid special attention to the subject, denied that iron was capable of alloy with titanium at all.

The matter has been tested latterly upon a tolerably large scale as regards crude pig iron at Middlesboro', where importation was made of some cargoes of Norwegian rutile, or natural titanic acid, and a very remarkable massive black titanate of iron; and those were employed along with the Cleveland ores. The following is the composition of the Norway ore in two samples by Mr. Betley's analyses :—

	No. 1.	No. 2.
Magnetic Oxide Iron, . .	46·14	54·72
Titanic Acid, . . .	36·88	40·80
Silica, ...	13·32	1·58
Magnesia, . . .	2·07	2·13
Lime, ...	0·78	0·66
Iron Pyrites ($Fe\ S_2$), . .	1·05	0·00
	100·24	98·99
Yield of Metallic Iron, . .	33·39	39·62

Neither contained phosphorus.

Rutile can be imported into Middlesboro' at £10 per ton, and these titanates at about thirty shillings per ton.

From Iceland, Sicily, the North-West of Neapolitan Italy, and other volcanic regions, if it were of any use, titanate of iron could be had for the mere cost of collection and freight home.

Iron made with red hæmatite and a mixture of bog iron ore from Ireland, with 7½ per cent. of these Norwegian minerals, did in some instances yield to Mr. Riley on analysis as much as 3·51 per cent. of titanic acid, but they contained *none* unless the Irish bog ore had formed part of the mixture.

More recently trials have been made with mixtures of Cornish ores, Irish bog ore, and red hæmatite, and the pig was found to contain *some* titanium.

The Cornish ores Mr. Betley found, were—

	No. 1.	No. 3.
Silica, . .	23·38	27·18
Peroxide Iron. .	41·96	47·32
Peroxide Manganese, .	25·77	16·35

with a little phosphoric acid.

From these the iron obtained had the following composition :—The bog ore employed contained 7 to 9 per cent. of manganese :—

Pig Iron.	No. 1.	No. 2.	No. 3.
Graphite, . .	3·120	3·010	2·615
Carbon combined, .	0·310	1·020	0·074
Silicon, . .	2·590	2·559	3·325
Iron, . .	89·900	86·880	84·256
Manganese, . .	5·850	6·370	8·607
Nickel and Cobalt. .	0·069	0·110	. .
Copper with Antimony, .	0·060	0 045	0·064
Phosphorus, . .	0·147	0·154	0·201
Sulphur, . .	0·026	0·026	0·017
Titanium, . .	0·799	1·150	0·629
	100·853	101·315	100 268

We have here, therefore, something above $1\frac{1}{2}$ per cent. of titanium, but the manganese is present in form nearly 6 to above 8 per cent. also.

Mr. Riley remains in doubt whether the titanium, even here, is alloyed or only minutely diffused, in an amorphous state, through the pig iron.

The repetition of analyses of M. Mushet's steel, failed in his hands to give any evidence of the presence of titanium. He remarks that the use of titanic ores involves that of a considerable addition of an easily fusible silicate, as a flux in the blast furnace, for otherwise the reduction is difficult, and the cinder tough and ropy, and hard to separate from the metal. Mr. Riley is prepared to admit, however, that the presence of titanium in iron ores does not appear to impart a steely nature to the iron made; and, in conclusion, so far, on a subject which he considers still demanding further research, he expresses his opinion that probably the part of titanium in iron is limited to its acting as a carrier of cyanogen to the iron with which it is mixed or combined, in the process of its conversion into steel, which seems to be supported by the ascertained affinity of titanium for nitrogen and carbon. To us it appears extremely doubtful still, that it exercises any influence whatever, for good or for ill, and that the large per centage of manganese present almost wherever titanium has been detected, has played the really important part in facilitating or improving the production of the steel.

The power of manganese in this respect is ascertained, although no doubt, as yet, much remains to be learnt as to its mode of action, and which will probably only be cleared up when the action of nitrogen in the production of steel shall have been better studied and understood.

In concluding this *resumé* of Mr. Riley's interesting researches, we may remark that there are in the South of Ireland enormous deposits (not bog ores) of poor ores of manganese, that may be viewed as very pure and highly manganesiferous iron ores.

These we should deem far more worthy of metallurgic trials upon the great scale, for the production of British pig of the *spiegeleisen* class, fitted for the Bessemer and other processes of steel-making, than probably any other in Great Britain.—Ed.

On a Curious Instance of Electrolytic Action. By F. A. Abel, F.R.S.

From the Journal of the Chemical Society, July, 1863.

The inspection of a number of the lead-coated projectiles employed with the Armstrong guns, has recently brought to light some curious and quite unexpected instances of the establishment of voltaic action within the coatings of certain of these projectiles, and in positions where the existence of an exciting cause was not at all anticipated.

It has been customary to attach the coating of soft-metal (which is an alloy of lead with a small proportion of tin or antimony) to the body of the cast iron projectiles, by two different methods. The one consists in providing the iron surfaces with a number of grooves formed at somewhat acute angles with the surface; the soft metal with which these become filled when it is cast over the shot or shell, serves to attach the coating firmly to the body. The other method consists in first alloying and coating the surface of iron with zinc (by the so-called galvanizing process), and then immediately covering it with the soft metal. The covering of zinc fixed, in this instance, upon the iron, becomes the medium by which the coating of lead-alloy is attached to the body of the projectile. By the latter method, therefore, a perfect juncture is accomplished between the two parts, while, by that first-named, they may be said to be fitted together accurately, or riveted together.

The different manner in which the iron and the lead-alloy are affected by considerable changes of temperature has led, in a few instances, of a very special character, to a distortion of the coatings which have been attached by the mechanical method (*i. e.*, by means of grooves) in consequence of their partial separation from the body of the projectiles. The alterations in form presented by some shells of this class, which have been exposed for a time to the effects of considerable changes of temperature, differ, however, altogether from those exhibited by a small proportion, among a number inspected, of shells, the lead-coatings of which were attached by means of zinc. On various parts of these shells, the soft metal was found to have become raised in the form of blisters, varying in size from $\frac{1}{4}$-inch to 1-inch in diameter. A large (110-pr.) shell, which had been preserved officially, as a standard pattern, since November, 1861 (having been kept in a glass case), exhibited, upon its coated surface, in addition to numerous smaller blisters, one measuring 1·1 inch in diameter, and projecting $\frac{1}{4}$-inch beyond the surface of the shell. These projections, or blisters, were not confined to any particular position upon the different shells; neither had those projectiles, which exhibited them, been exposed to any considerable changes of temperature.

The only inference deducible from the appearance of these projectiles was, that the blisters had been produced in consequence of the generation of gas at those parts of the shells between the iron surface and the coating of soft metal, which gas had gradually accumulated to such an extent as to be placed under very considerable pressure, and, consequently, to exert, eventually, an amount of force sufficiently great to tear asunder the two surfaces of metal which had been joined by means of the zinc, and to raise up and expand the comparatively plastic alloy of lead. This influence was fully borne out by the following results of examination of the blisters.

When these were punctured, under water, a quantity of gas, existing evidently under considerable pressure, made its escape. The gas was separately collected from blisters upon different shells, and was found, in all instances, to consist of pure hydrogen.

The entire volume of gas, enclosed in the large blister specially referred to above, was carefully collected, and its volume was determined. The capacity of the blister was afterwards measured, and the results furnished by these determinations showed that the hydrogen existed at the period of examination, under a pressure very slightly exceeding ten atmospheres.

By carefully cutting through the metal surrounding the blisters, several, which had been otherwise operated upon, were removed from the shells, the internal surfaces remaining untouched. On a close inspection of these surfaces, they were all found to resemble each other closely, those of the blisters as well as those of the iron body of the shell, immediately beneath them. They presented a frosted metallic appearance, small dark patches or spots, indicative of corrosion, being visible here and there. Those of the blisters and of the iron beneath were alike coated with zinc, and their peculiar appearance, just referred to, was to be ascribed to the existence upon them of numerous small projections, such as are observed upon the surfaces laid bare when a piece of metal is fractured by a tensile strain.

It is evident, from the foregoing details, that the projections, or blisters, which have been described, had been produced by the gradual disengagement of hydrogen between the body of the projectile and the coating of lead-alloy, whereby the two descriptions of metal, joined together thoroughly in the first instance (except probably at a few minute points) through the medium of zinc, had been torn asunder and the thin intermediate zinc coating itself divided, the gas gradually creating further space for itself by expanding the envelope of soft metal which confined it.

That these results must have been due to the electrolytic decomposition of some hydrogen-compound, which had become enclosed between the metals, is self-evident; an examination into the process of manufactures of these projectiles, aided by a few simple experimental data, appears to leave no doubt regarding the primary cause of this disengagement of gas.

The operations included in the system of coating the iron projectiles by the aid of the so-called galvanizing process, are as follow:—The cast-iron projectile, after having been turned, so as to present a perfectly clean metallic surface, is heated in an oven until its temperature approaches the fusing point of zinc; it is then plunged into a solution of sal-ammoniac, almost immediately withdrawn, and immersed in a bath of melted zinc, where it remains for about two minutes, when it is transferred to a bath of the lead-alloy with which it is to be coated. After remaining in this bath also for a period of two minutes, it is withdrawn and placed in a mould. The projectile thus treated has become uniformly covered with a thin coating of soft metal, the thickness of which is afterwards increased, by pouring a further quantity of the alloy round the coated projectile, directly it has been placed in the mould.

When the heated projectile is passed from the chloride of ammonium solution to the bath of zinc, portions of the salt remain adhering to the iron, upon its immersion in the zinc, and there is no doubt that a small proportion of water may also, occasionally, be carried along with it, so as to be brought together with it into contact with the melted metal. A small proportion of chloride of zinc is always formed at this stage of the process, by decomposition of the chloride of ammonium, in consequence of the high temperature at which particles of this salt are brought into contact with the zinc, and with oxide of zinc, small portions of which may accidentally attach themselves, at the moment of immersion, to any slight inequalities on the surface of the iron. The remarkable tendency of chloride of zinc to absorb and retain water, even at very high temperatures (the latter being only gradually expelled from it, even at the temperature of melted zinc), renders it easily conceivable that, at the time of production of the zinc-salt, any water accidentally present, besides that which results from the decomposition of the ammonium-salt by oxide of zinc, will at once be absorbed by it; there is no doubt, therefore, that any particles of chloride of zinc accidentally enclosed by the zinc-coating formed upon the iron surface, will still retain some water at the expiration of the brief period during which they continue exposed to the high temperature of the zinc-bath. Afterwards, when the coating of zinc becomes, in its turn, speedily enveloped in that of the lead-alloy, the temperature of the projectile gradually diminishing up to the termination of the process, the escape, or decomposition, of the water through the agency of heat, is speedily arrested.

Thus it is that portions of water become accidentally enclosed between the iron body and the zinc-coating of compound projectiles prepared by this system; and it is this enclosed water which afterwards suffers gradual decomposition through electrolytic agency; the accumulation of the liberated hydrogen giving rise, eventually, to the projections or blisters in the coating of soft metal.

The surfaces laid bare by carefully cutting open many of these blisters, invariably furnished proof that chloride of zinc had been enclosed at those places, and that a basic chloride had been formed. Water removed from them a mere trace of chlorine, but, by a brief treatment with dilute acetic acid, abundant evidence was obtained of the existence of a chloride within the blisters.

It should be observed that these accidental distortions of the lead-coating on Armstrong projectiles which have been prepared by means of the galvanizing process, appear not to be of frequent occurrence, and that they do not interfere with the efficiency of such projectiles, because, even in the most striking instances, they constitute only a very small proportion of the coating of soft metal, which is in all other parts inseparably joined to the iron; and also, because the coating may, in places where it has been raised in blisters, be restored to its original dimensions by simple mechanical means.

Translated for the Journal of the Franklin Institute.

Relative Effect of different Points upon Lightning-Rods.

[Note addressed by M. Perrot, to the Academy of Sciences at Paris.]

By means of an electric machine, I charged a large metallic plate representing a cloud, until a very sensitive electrometer marked 10°. I then gradually approached to the plate, first a rod rounded at its extremity, such as M. Despretz has proposed as the terminal of a lightning rod; afterwards an ordinary lightning-rod point, and finally a very sharp point. These experiments gave me as a mean, the following results:

1. The round-pointed rod remained without neutralizing effect until it was struck, at a distance which I shall assume as the units.
2. The neutralizing action of the common point did not begin until the distance was less than 12 units.
3. At the distance of 12 units, at which the common point had no neutralizing action, the sharp point discharged the plate instantaneously.
4. The neutralizing action of the sharp point began to show itself when the distance was less than 170 units.

The neutralizing action of the fine point, therefore, extended nearly 170 times further than the striking distance, or 13 times further than the action of the common point.—*Comptes Rendus.*

Translated for the Journal of the Franklin Institute.

On the Preservation of Animal Matters in the open air.

By M. Pagliari.

I have the honor to communicate to the Academy (*of Sciences at Paris*), a new and very simple mode of preserving animal matter. The liquor which I employ for this purpose, is composed of alum, benzoin and water, and differs but little from my *hœmostatic water.* A single coat of this preserving liquid applied upon the substance which is then exposed to the open air, is sufficient to prevent its decay.

Comptes Rendus.

Experiments on Steel Steam Boilers.

From the Lond. Practical Mechanic's Journal, April, 1864.

Some interesting experiments have recently been made in Prussia with steel steam boilers.—A steel steam boiler of the egg-ended shape, 4 ft. in diameter and 30 ft. in length, without flues, was tried. It had a steam drum 2 ft. in diameter and 2 ft. in height, and the plates were one-fourth of an inch in thickness. Beside it there was placed another boiler similar in every respect, excepting that the plates were of iron 0·414 of an inch in thickness. The steel boiler was tested by hydraulic pressure up to 195 pounds on the inch without showing leakage, and both the iron and steel boilers were worked under a pressure of 65 pounds on the inch for about one year and a half. During this period, the steel boiler generated 25 per cent. more steam than the iron one, and when they were thoroughly examined after eighteen months practical working, there was less scale in the steel than in the iron boiler. The former evaporates 11·66 cubic feet of water per hour; the iron boiler 9·37 cubic feet. The quantity of coal consumed was on an average 2706 pounds for the steel one in twelve hours, and 2972 pounds for the iron boiler. The plates of the steel boiler over the fire were found to be uninjured, while those of the iron one were about worn out. In Prussia several worn-out plates of iron boilers have lately been replaced with steel, which, it is stated, lasts four times as long. As steel is twice as storng as iron, thiner plates of the former may be employed for boilers, and more perfect riveting can be secured. A greater quantity of steam can also be generated in the steel boiler on account of its thin plates, and thus much fuel may be economized. Such steam boilers should engage the attention of all who make and use steam boilers for engineering and manufacturing purposes.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

From the Journal of the Society of Arts, No. 558.

[Report of the Chief Engineer for May, 1863.]

During the past month there have been examined 257 engines—1 specially; 420 boilers—10 specially, 11 internally, 77 thoroughly, and 322 externally; in addition to which, 1 of these boilers has been tested by hydraulic pressure. The following defects have been found in the boilers examined:—Fracture, 8 (3 dangerous); corrosion, 22; safety-valves out of order, 1; water-gauges, ditto, 11; pressure gauges, ditto, 6; feed apparatus, ditto, 2; blow-out apparatus, ditto, 3; fusible plugs, ditto, 1; furnaces out of shape, 2 (1 dangerous); over-pressure, 2; (both dangerous); blistered plates 2. (1 dangerous); total, 60 (7 dangerous). Boilers without glass water-gauges, 2; without pressure-gauges, 5; without blow-out apparatus, 17; without back pressure-valves, 34.

The occurrence of explosions during the past few months has been so frequent, that the reports of their details have prevented any notice of the defects found to exist in the boilers under inspection, further than that given in the monthly list as above; and, therefore, the present opportunity may be taken for some slight amplification.

Serious cases of corrosion continue to be met with at the bottom of those boilers which are set upon brick mid-feathers, running underneath them from one end to the other along the centre or keel line. In this mode of setting, any water which may fall upon the boiler, or leak from the seams, trickles down the shell, and settles on the top of the mid-feather, in contact with the plates. Also where damp exists in the flues, it easily rises through the mid-feather and reaches the boiler. Corrosion may be going on along the centre of the mid-feather and not be visible at the sides, and thus pass undetected even on careful examination—several instances of which have recently been met with.

In one case, a boiler 10 feet 5 inches in diameter, and of plates seven-sixteenths thick, was found, upon the removal of the brick-work, to be deeply channelled for a width of six inches along the bottom at the centre of the seating, no indication of which was given at the sides of the wall, nor was it detected by hammering internally, but remained unknown until the removal of the brick-work. In another boiler, the shell of which was 8 feet 2 inches in diameter, and three-eights in thickness, the plates were found to be channelled longitudinally along the centre of the mid-feather for a width of eight inches. The seating was fifteen inches wide, and the channel died out at three inches from the outside, thus giving no external sign. In a third boiler, the flues of which were damp, the whole of the plates in contact with the mid-feather were found to be corroded, and as much as a quarter of an inch in thickness eaten away. Other instances might be adduced, but the character of the injury is so similar in each case that it would be tedious. Those already given will show the grounds upon which the following recommendations are made:

First. Wherever it is practicable to do so, dispense with mid-feathers altogether, and substitute what are termed two side walls for them. Second. In those cases where the small size of the boiler forbids this, make the bearing surface of the mid-feather wall as narrow as possible. Third. Any that must be retained in use, should, in preparation for annual "thorough" inspection, be removed—at least where in contact with the transverse seams of rivets—in order to admit of complete examination.

Further cases of corrosion may be mentioned occurring to other boilers than those set upon mid-feathers.

A tubular boiler, without any external flues, was found so deeply channelled at the bottom of the shell at the transverse seams of rivets, that the plates were reduced to the thickness of a sheet of paper, and a hole knocked through them in sounding their strength with a hammer. A second boiler, of ordinary double furnace, internally-fired construction, was so eaten away by corrosion at the first bottom plate from the front, that, as in the preceding case, a hole was made on examination, the corrosion being due to leakage from the glass water-gauges and mudhole joint, as well as to the practice of slacking the ashes while lying at the front of the boiler. In another case, where the cross wall below the front end of the boiler was as much as two feet in thickness, the plates were corroded nearly through. So great a thickness as two feet for these walls is quite unnecessary, as well as objectionable, and it is recommended that they should be removed periodically for the examination of the plates.

Several cases have been met with during the past month of injury occurring to externally-fired boilers at the parts immediately over the furnace, the plates bulging, cracking at the rivet holes, and the seams straining and leaking. One of our members has contributed a sample plate cut from a boiler of this class, which is bulged down at the solid, and cracked completely through, although the boiler was amply supplied with water; this plate is a fair sample of the danger of the externally-fired class.

A safety-valve, the spindle of which passed through a bushed hole in the cover, was found to fit so tight as to be quite fast. This bush had been fitted to prevent the inconvenience arising from the escape of steam, under the impression that a safety-valve was unnecessary, as long as the boiler was provided with a steam pressure-gauge.

A report relative to explosion, No. 9, recorded in the last month's abstract, has since been received from an engineer who examined the boiler shortly after the explosion had occurred, and from which it appears that the boiler, which was of plain cylindrical egg-ended construction, and externally fired, had rent—as is so constantly found to be the case in this class—at the transverse seams over the fire, the shell dividing into two parts, which were thrown some distance asunder.

One boiler only, and which was not under the inspection of this association, has been reported to have exploded during the last month; the explosion occurring at too great a distance from Manchester to admit of a personal investigation. Should any particulars of interest be ascertained, they will be communicated in the next monthly report. The explosion will rank as No. 10 in this year's report.

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[Report for June, 1863.]

From the Journal of the Society of Arts, No. 562.

During the past month there have been examined 340 engines—2 specially; 528 boilers—10 specially, 12 internally, 104 thoroughly, and 402 externally, in which the following defects have been found:—Fracture, 8 (2 dangerous); corrosion, 34 (3 dangerous); safety-valves out of order, 9; water-gauges ditto, 21; pressure-gauges, ditto, 9; feed apparatus, ditto, 2; blow-out apparatus, ditto, 35; fusible plugs, ditto, 13; furnaces out of shape, 4 (2 dangerous); over pressure, 2 (both dangerous); blistered plates, 3; total, 140 (9 dangerous). Boilers without glass water-gauges, 8; without pressure-gauges, 2; without blow-out apparatus, 13; without back pressure-valves, 39.

Explosions.—No. 10 Explosion :—The fact of this explosion having occurred, was stated in last month's report, but no detailed particulars had then been received. It has since been ascertained that the boiler was a plain cylindrical egg-ended one, externally-fired, and that the explosion was caused by rents commencing at the seams over the furnace, which has so frequently been found to be the case in this class of boiler, and called attention to in these reports. Two other explosions of a very similar character have happened during this month, particulars of which are given.

Nine explosions have occurred during the last month, by which eleven persons have been killed, and twenty-one others injured. One of these explosions, which resulted from collapse of the flue, and by which no injury was done either to persons or surrounding property, occurred to a boiler under the inspection of this Association; while in the remaining cases not one of the boilers was under its care. Details will be found below.

The following is the monthly tabular statement :—

TABULAR STATEMENT OF EXPLOSIONS FROM MAY 23, 1863, TO JUNE 26, 1863, INCLUSIVE.

Index No.	Date.	General Description of Boiler.	Persons killed.	Persons injured.	Total.
No. 11.	May 29.	Locomotive, . . .	1	1	2
No. 12.	" 30.	Cylindrical egg-ended externally-fired,	5	12	17
No. 13.	June 2.	Upright furnace internally-fired, .	0	0	0
No. 14.	" 13.	Details not yet ascertained. . .	1	5	6
No. 15.	" 13.	Ordinary single flue, or "Cornish" internally-fired, . . .	0	0	0
No. 16.	" 14	Cylindrical egg-ended externally fired,	1	0	1
No. 17.	" 15.	Details not yet ascertained, . .	2	3	5
No. 18.	" 15.	Marine, . . .	1	0	1
No. 19.	" 26.	Details not yet ascertained, . .	0	0	0
	Total .	. . . . .	11	21	32

No. 11 Explosion occurred to a boiler of a locomotive engine while running with a passenger train. Between fifty and sixty persons were injured and four killed. This result was not occasioned, however, by the explosion only, but principally by the train running off the line. Whether the engine leaving the rails was the cause or the effect of the explosion, is an investigating question, and one now undergoing strict investigation. All the facts likely to prove of value to steam users generally, which may be elicited with regard to this explosion, will be given to the members of the Association at the earliest opportunity after the close of the inquiry. The only injuries directly attributable to the explosion, are those which happened to the engine-driver and

fireman, both of whom are severly scalded, the latter having died in consequence.

No. 12 Explosion, from which five persons were killed and twelve others injured, took place at an iron-works. The boiler in question was not under the care of this Association, was personally examined shortly after the explosion happened, and found to be of cylindrical egg-ended construction, having an internal flue of horseshoe-shape, both the inlet and outlet of which passed through the further end of the boiler, the remainder of the flue being quite independent of the shell, and thus not forming any longitudinal tie from front to back. The boiler was externally-fired, the flame passing underneath the shell, and then entering the flue at one end of the horseshoe, and escaping to the chimney through the other. The length of the shell was twenty-eight feet, the diameter eight feet six inches, and the thickness of the plate seven-sixteenths, while the blowing off pressure was about 40 lbs.

The boiler had rent completely into two parts at the fourth transverse seam from the front end, the larger portion of the shell flying forwards in a straight line from its seat, turning a summersault in its course, and landing in a position quite the reverse of its original one; the egg-ended pointing to the brick-work seating, and the open one from it. The smaller portion had flown to a much greater distance than the other, and not, as is usually the case, in a direction immediately opposite, but at right angles to it. A sister boiler working alongside, and connected to the one in question, was moved laterally, sufficiently so to disturb the brick-work seating and break the steam-pipes, though not to unseat it altogether.

With regard to the cause of the explosion :—The boiler was fifteen years old, the plates over the furnace had already been repaired, and it was stated that the seams at that part had been observed to be leaking only a quarter of an hour before the explosion took place: while, in addition, the shell was found to be patched in several places, and the plates cracked from the rivet holes to the edge. It is concluded, therefore, on consideration of all the circumstances, that the boiler could not have been in good condition, the correctness of which it is thought derives some corroboration from the fact, that the boiler alongside was found at the time of making this examination, to be also leaking at the seams over the furnace, and that considerably, although not under pressure. An examination of the fractures, as well as a consideration of the direction in which the parts had flown, led to the conclusion that the rent had commenced at a longitudinal seam of rivets, extending for some two or three plates over the furnace. The rent ran along as far as this longitudinal seam extended, and when met by a plate crossing it, or "breaking-joint," as it is termed, then developed into a transverse rent, and completely severed the shell in two. It is thought that the fact of this longitudinal rent in the furnace-end of the shell being situated on one side of the centre, or "kell-line," accounts for that portion having been blown laterally, and that the upward direction which it had evidently taken, had caused the summersault of the remaining and larger portion. The explosion, therefore, is attributed to the imperfect condition of the boiler, and although such defects would not be dangerous in a suitably-constructed double-flued, or "Lancashire" boiler, which is always internally-fired, they are generally found to be fatal in those boilers which are fired externally; while in the present case the effect was aggravated by the fact of the seams of rivets over the fire being in line, and the diameter of the shell being as much as eight feet six inches.

No. 13 Explosion, by which no one was injured or the surrounding property damaged, occurred to a boiler under the inspection of this Association, the particulars of which are as follows:—

The boiler was an upright furnace one, working in connexion with two others of similar construction to itself. It was heated by the flames passing off from a furnace employed in preparing heavy forgings, the flames passing through an internal tube in the centre of the boiler, which ran directly from the top to the bottom. The extreme height of this internal flue was twenty-six feet two inches, but it was not of one diameter throughout. In order to admit of a brick-work lining to guard the flue above water line, the upper part was made of a larger diameter than the remainder, and attached to it by a flanged plate, which formed a "set-off," or shelf, on which the brick-work rested; again, the lower portion of the tube had a bell-mouth at the bottom, to afford an easy entrance for the flame. The length of the upper part was 11 ft. 4 in., and the diameter 3 ft. 3 in.; the length of the intermediate portion was 10 ft. 4 in., and the diameter 2 ft. 6 in., while the length of the bell-mouth was 4 ft. 6 in., and the diameter at the base 3 ft., the thickness of the plates being three-eighths of an inch throughout, and the blowing-off pressure 55 lbs.

The explosion, which did not in any way disturb the original position of the boiler, resulted from collapse of the internal flue tube, the collapse being confined to the intermediate portion just described, which it rent about the middle of its length.

A tube of such small dimensions as those just given, namely, only 10 ft. 6 in. in length, and 2 ft. 6 in. in diameter, made of plate three-eighths in thickness, if of good material and workmanship as this one was, would be amply sufficient for a pressure of 55 lbs., if working under ordinary circumstances. This would suggest the conclusion that the water supply had been allowed to run short, but no positive indications of the plates having been over-heated appeared upon examination, though this may, however, have taken place on previous occasions without its being known. On account of the height of these upright furnace boilers, the glass gauges became inaccessible, and the one in question was fitted with two gauge-taps only, carried down by means of syphon pipes to within reach from the floor. Thus the water could sink below the proper level without affording any external indication, and would consequently pass unknown, should the gauge-taps be neglected. This may have happened without any immediate collapse of the tube taking place, although the flue would be materially weakened by it, and rendered liable to give way some time after in consequence. It is impossible to say whether the flue tube was getting out of shape or not, since the boiler had been in such constant work, that no opportunity was afforded the Association of making an "internal" and "thorough" examination for upwards of three years, This may not, therefore, be an improper time for calling attention to the importance generally of having spare boilers, so that a suitable opportunity may be afforded for examination, as well as for cleaning and repair. The boiler was found to be heavily incrusted with hard scale, which must considerably have tended to the over-heating and weakening of the flue, to which these vertical boilers are always prone, from the tendency of the ascending steam to cling around the tube and prevent the contact of the water. The inaccessibility of the gauges and fittings of these vertical boilers, on account of their height, is another disadvantage connected with them, and, indeed, a thoroughly good and safe furnace boiler must still be considered as a desideratum.

No. 16 Explosion happened at an iron-works, to an externally-fired boiler 40 ft. long, 8 ft. in diameter, made of plates seven-sixteenths of an inch thick. The shell of the boiler gave way immediately over the furnace, the fireman being scalded to death from the stream of hot water issuing from the rent. The boiler had been repaired at this part, by putting on a new plate two months previous to explosion, and it was at this plate that the rent occurred.

This boiler, which was not under the charge of this Association, was not personally examined on the occurrence of the explosion, but an engineer who inspected it shortly after, has kindly furnished the following particulars:—The plate ripped open through the solid metal in two places, the rents being about fifteen inches long and one inch wide, while the parts surrounding it were a good deal cracked and the seams patched, so that the bottom of the boiler was evidently in a very defective state. The ruptured plate was about seven-sixteenths of an inch thick, and did not appear of very good quality, as if not thoroughly welded in rolling; but the fractures were not, properly speaking, "blisters," since the whole thickness of the plate had come down at once. In addition to the inferior character of the plate, the boiler was heavily coated with incrustation, and this had accumulated at the bottom for a depth of three inches just over the fracture, and extended for a space of four feet by two feet. This mud had hardened so much that the water and steam ploughed but a small hole through it in rushing out. There can be little question that this coating of mud had kept the water away from the plate, and thus lead to its being over-heated, from which it gave way. There was plenty of water in the boiler at the time of the rupture.

It will be clear that it is scarcely possible for such a cake of sediment to have formed upon the furnace crown of an internally-fired boiler, and also that an efficient blowing-out apparatus would have prevented the accumulation in a great measure, if not altogether, and thus that the explosion was due to the dangerous character common to all externally-fired boilers, coupled in this case with neglect.

For the Journal of the Franklin Institute.

The Sphere, Cone, and Cylinder.

In an old book, entitled "The Elements of Euclid, with select theorems out of Archimedes," printed in London in 1719, I find data, which, by the aid of our tables of areas of circles, will facilitate computations of the surfaces and solidities of spheres, spherical segments and sectors, and the comparative superficies and solidities of inscribed and circumscribed cylinders and cones.

1st. To find the surface of a sphere:—"Let the greatest circle of the sphere be measured, and let it be multiplied by 4." It is evident from this that the area of a circle whose diameter is double that of the given sphere must be equal to the surface of the sphere. To shorten the work of calculation, double the diameter of the given sphere, and from the tables take the circle area corresponding for the surface of the sphere.

2d. To find the solidity of a sphere:—"If the 6th part of the diameter be multiplied by the surface of the sphere, there will arise the solidity of the sphere." Doubling the given diameter and taking its 6th part can be done in the mind in most cases, while that part left for the pencil is simply the multiplication of the tabular area by the 6th part of the diameter; thus, suppose a 15-inch sphere is given to find the solidity.

Area of 30-inch diameter $=706{\cdot}86=$ surface of sphere,

and $706{\cdot}86\times 2{\cdot}5=1767{\cdot}15=$ solidity required.

3d. To find the surface of a spherical segment or zone. It is proven, that the surface of a spherical segment or zone, is to the entire spherical surface of which it is a part, as the height of the segment is to the diameter of the sphere. From this proportion the rule is plain. Thus:—Multiply the entire spherical surface by the height of the segment and divide by the diameter of the sphere.

4th. To find the solidity of a spherical segment or sector. Multiply the spherical surface of the sector by the third part of the radius for the solidity required, and for a segment find the solidity of the sector of which it forms a part, then deduct the cone which lies between the base of the segment and centre of the sphere, if the segment be less than a hemisphere, and add it thereto if the segment be greater.

5th. "The superficies of a right cylinder (one whose height and diameter are the same,) circumscribed about a sphere is equal to the surface of the sphere." The cylindrical surface only is here meant.

"If a cylinder and inscribed sphere be cut by planes perpendicular to the axis, each segment of the cylindrical surface will be equal to the corresponding segment of the sphere."

From this property the 3d rule above is evident.

6th. "A right cylinder is both in solidity and the whole superficies to the sphere about which it is circumscribed, as 3 to 2."

7th. "The superficies of a sphere is to the whole superficies of an equilateral cone circumscribed about it as 4 to 9."

8th. "The whole superficies of an equilateral cone circumscribed about a sphere is quadruple of the whole superficies of an equilateral cone inscribed in the same sphere, and the solidity of the former cone is eight-fold that of the latter."

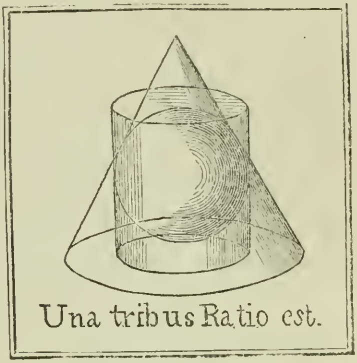
9th. "A sphere hath that proportion to an equilateral inscribed cone as 32 to 9."

10th. "A sphere hath the same proportion both in respect of solidity and surface to the equilateral cone circumscribed about it which 4 hath to 9."

11th "A sphere, a circumscribing cylinder and a circumscribing equilateral cone, both with respect to solidities and whole superficies, bear the continued proportion, respectively, to one another, as 4 : 6 : 9."

Archimedes set great value upon his discovery of the sesquilateral proportion of the right cylinder and internal touching sphere, as enunciated in the 6th property above; such that he would have a figure of it engraved upon his tomb.

It is a matter of argument why he should prize this above other discoveries which he made, and above machines put into practical use, and which would seem to have given him a wider fame than the discovery of any abstruse mathematical relations or inherent properties of lines and surfaces.



There is one ratio for the three.

But so it was, he valued the discovery of purely mathematical fact far above any practical display of prepared matter, or mechanical combinations, whatever may have been the efficiency of its performance or the prodigiousness of the force which it developed.

Archimedes' discovery received a very important as well as interesting addition at the hands of the learned Andrew Tacquet, the author of the old book in question, and the two combined are given in the 11th property above with a figure, inscription, and translation.

It is singular indeed that bodies whose surfaces and sections present so many difficulties in practice should in themselves, as wholes, bear so simple a relation, one to another. Y.

On the Employment of Burning Wire, as an Illuminating Agent, Especially for Photographic Purposes. By Professor Roscoe.

From the London Practical Mechanic's Journal, April, 1864.

Professor Roscoe exhibited the light emitted by burning a portion of a fine specimen of pure magnesium wire 1 mm. in diameter and 10 feet long, which had been manufactured by Mr. Sondstadt. Professor Roscoe remarked that it afforded him great pleasure to be able to state that a suggestion made by Professor Bunsen and himself in their photochemical researches, and printed in the Philosophical Transactions for 1859, page 922, was about to be practically adopted. Mr. Sondstadt is now commencing to manufacture the metal magnesium on the large scale, and the first important application of the metal is the employment of burning magnesium wire as an illuminating agent, especially for photographic purposes. In the researches above mentioned, Professor Bunsen and the speaker had examined the photochemical action of the sun compared with that of a terrestrial source of light, and for the purpose of this comparison they chose the light evolved by the combustion of magnesium wire. They showed that a burning surface of magnesium wire which, seen from a point at the sea's level, has an apparent magnitude equal to that of the sun, effects on that point the same chemical action as the sun would do when shining from a cloudless sky at a height of 9° 53′ above the horizon. On comparing the *chemical* with the *visible* brightness of these two sources of light it was found that the brightness of the sun's disk as measured by the eye when the sun's zenith distance was 67° 22′, is 524·7 times as great as that of the burning magnesium wire, whilst, at the same zenith distance, the *chemical* brightness of the sun is only 36·6 times as great. Hence the value of this light as a source of the chemically active rays for photographic purposes becomes at once apparent. The extract from the memoir referred to is as follows:—

"The steady and equable light evolved by magnesium wire burning in the air, and the immense chemical action thus produced, render this source of light valuable as a simple means of obtaining a given amount of illumination expressed in terms of our measurement of light. . . . . . The combustion of magnesium constitutes so definite and simple a source of light for the purpose of photochemical measurement, that the wide distribution of this metal becomes desirable. The application of this metal as a source of light may even become of technical importance. A burning magnesium wire of the thickness 0·297 millimetre evolves, according to a measurement we have made, as much light as 74 stearine candles of which 5 go to the pound. If this light lasted one minute, 0·987 metre of wire, weighing 0·1204 grm., would be burnt. In order to produce a light equal to 74 candles burning for 10 hours, whereby about 20 lbs. of stearine is consumed, 72·2 grms. of magnesium would be required. The magnesium wire can be easily prepared by forcing out the metal from a heated steel press having a fine opening at bottom; this wire might be rolled up in coils on a spindle, which could be made to revolve by clock-work, and thus the end of the wire, guided by passing through a groove or between rollers, could be continually pushed forward into a gas or spirit-lamp flame in which it would burn."

Professor Roscoe stated that great credit was due to Mr. Sondstadt for the able manner in which he had brought the difficult subject of the metallurgy of magnesium into the present satisfactory position, and expressed his opinion that, even in photographic purposes, the application of the metal will prove most important. Mr. Brothers, Mr. Parry, and other photographers present, corroborated Dr. Roscoe's opinion respecting the value of such source of light for photography. Since the meeting Mr. Brothers made an experiment upon the magnesium light, which he reports as follows:—

"The result of an experiment I have just tried is that in 50 seconds with the magnesium light I have obtained a good negative copy of an engraving—the copy being made in a darkened room. Another copy was made in the usual way in daylight, and in 50 seconds the result was about equal to the negative taken by the artificial light. The sun was shining, but there was a good deal of fog in the atmosphere."

To the Editor of the Journal of the Franklin Institute.

Sir:—The propriety of introducing the word "equivalent," in patent claims, has been a subject of much discussion and difference of opinion among both patentees, and the officials of the Patent-Office.

The following argument was written with the hope of obtaining an official decision which would set the question at rest for the future. It was submitted to the Commissioner of Patents, who, by granting the patent, acknowledged the pertinence of the grounds advocated in the argument.

Argument in the matter of the Application of R. Heys *for Letters Patent for Variable Valve Motion for Steam Engines.*

To the Honorable Commissioner of Patents.

Sir:—The contents of the official letter of December 7th, 1863, have been carefully perused, and the suggestions of the Examiner in charge of this case duly considered.

The official letter contains the following:—"It is well understood that the law regards real equivalents as secured to you without the use of the language (the equivalents to the same) in your claim. It is therefore regarded as superfluous."

I regret that my opinion on this point is at variance with that of your able Examiner.

The word "equivalent," in the claim of a patent, is, according to my humble views, not only not superfluous, but in very many instances a word of the most vital importance, a word, the insertion of which an applicant for a patent has a right to insist upon, unless the Office can give references of a character which demands the limiting of the claim to a special combination and arrangement of parts.

When an inventor presents an application for a patent for an improvement on the invention of another, it is the very proper practice of the Patent-Office, to require the applicant to word his specification in such terms as to acknowledge the rights vested in his predecessor.

So also has it been the practice of the Patent-Office, to allow broad claims for inventions for a novel character, and such words as *equivalent* and *substantially*, have been hitherto allowed for the express purpose of adding breadth to a claim.

The Office has invariably attached importance to the word equivalent, for in hosts of instances, specifications have been returned for the erasure of that word, because it rendered claims so broad as to conflict with those in prior patents, while in almost as many instances the insertion of the word has been permitted.

It is scarcely correct to say that "it is well known that the law regards real equivalents as secured without the use of the word equivalent in a claim:" this may be well known to those versed in patent matters, but a lengthened experience as a patent attorney has convinced me that the public is very far from being possessed of this knowledge, and for this reason it is respectfully contended that the word should be allowed whenever the nature of the invention will permit its insertion.

What better argument can be advanced in support of the use of this word than that the law allows it? What better guide can an applicant follow in framing his claim than the law?

Whatever strength the word equivalent may impart to a patent, and there can be no doubt that the word does impart more or less strength, there is an important reason why the word should be allowed on all appropriate occasions. Our community is unfortunately infested with a class of men who are a terror to meritorious inventors. I allude to the pirates or counterfeiters, who no sooner become aware of the value of an invention, which may be the successful result of long study, laborious research, and elaborate experiments, than they set whatever wits, or rather whatever low cunning they may have to work, with the view of infringing on the successful inventor's rights, evading his patent, or to use more simple and appropriate language, picking his pockets. Where is there a patent attorney of any lengthened experience who cannot count these pirates by the score, who cannot call to mind hundreds of instances in which these copyists have asked advice in somewhat such terms as follows:—"Mr. H., a patent has recently been granted to Mr. B., for an improved——. This is his claim; it is a very valuable patent, and there is a great profit in the article for which there is a great demand: I propose to make a somewhat similar invention; I use part of Mr. B.'s patent, but I substitute other devices for part of what he claims. Don't you think, Mr. H., that by so doing I can evade the patent granted to B., especially as his claim is limited to the particular arrangement or combination of parts?"

If all patent attorneys would treat such characters as they ought to be treated, that is, make such physical demonstrations as would insure their precipitate retreat, the evil might be remedied; unfortunately, however, the acts of these parasites are in too many instances encouraged. The best remedy, however, rests with the Patent-Office.

These piratical interlopers with a species of short-sighted cunning in almost every instance, base their operations on the wording of the claim of the patentee whose rights they propose to invade, and lay their plans accordingly.

If they peruse a carefully framed claim with the word *equivalent* or *substantially* appropriately introduced, they approach it with caution, they look upon it as property well fenced round, and in most cases abandon the idea of committing the premeditated act of piracy as too dangerous a proceeding.

If the meritorious inventor can by the use of the simple word *equivalent* in his claim keep the vermin at arms length, if the law allows the use of the term as it certainly does, if prior inventions present no bar to the introduction of the word, for heaven's sake let the inventor have it.

An applicant's claim should be framed in plain and concise terms; such terms as the most untutored mind can understand. "It is the duty," said Judge Story (Wyeth *vs.* Stone,) "of the patentee to sum up his invention in clear and determinate terms, and his summing up is conclusive upon his rights and title." "If the inventor (says Judge Grier) sums up the particulars of his invention, he is confined and held to such summary, and his patent must stand or fall by it."

If an invention consists of three things, A, B, and C, in combination with a fourth, which I will designate by the letter D, and if the three things are absolutely necessary to constitute the main features of the invention, while it is palpable to both the inventor and others that many things might be used to develop these features in place of D, why should not the inventor say so in his claim? why should he not say I claim A, B, and C, in combination with D, or its equivalent? Why should the Patent-Office object to such a claim because the law assumes the word equivalent to be understood although not expressed?

Is the praiseworthy and industrious inventor to be rewarded with a patent concluding with a claim which every one possessing common sense can understand, or is he to have a patent which appears to the envious and covetous pirate like an English act of Parliament, through which a member declared he could drive a coach and six, a patent which may form the nucleus of expensive lawsuits, a patent, the true meaning of which cannot be ascertained without an appeal to the law, or without resorting to the costly array of judges, juries, and venal professional experts?

In relation to this subject there is another important point to be considered.

It is an undoubted fact that there is a large, and I may add a respectable portion of our community who look upon patents and patent property as too precarious to meddle with, who consider the dealing in patents as closely allied to lottery transactions and gambling.

Ask these men the cause of their opinions, and in many instances you will find that either they or their friends have had their fingers bitten (to use a common phrase) by patents, their rights invaded, their profits squandered in lawsuits and their peace of mind destroyed by fruitless litigation.

The continued prosperity of the Patent-Office depends upon the value of patent property; the depreciation of this property soon tells on the receipts of the Office. If such claims only are allowed to an inventor as will leave his property open to the incursions of avaricious copyists, if the patents cannot be interpreted or sustained without resorting to expensive legal interpretations, patent property must soon become a drug in the market, inventors must cease to ply their vocations, the progress of the arts must be impeded, and not only the Patent-Office but the public at large must be sufferers.

In decrying the pirates who prey upon the industrious and ingenious inventors, let it not be supposed that I desire to confound these pests with a class of men who base their inventions on those of others, but are willing to admit the rights of their predecessors and to limit their claims accordingly. These men who may be termed real inventors are almost as important and valuable members of the community, as the originators, for it is by continued improvements only that true perfection can be arrived at, and I would here remark that patents of this class drawn up in proper candid terms are, as far as my experience goes, more valuable to their owners than such as are surreptiously obtained, and concocted from the brains of others.

To return to the present application which has given rise to the above remarks. It will be found that Heys claims the valve spindle D, link E, eccentric rod G and arm F, in combination with a weighted lever L and link *h*, or their equivalents. Now it is quite evident that D,E,G, and F, are absolutely necessary to carry out the applicant's intentions, and therefore any equivalent to them is not referred to: it is equally evident that many devices might be used in place of K and *h* for causing the motion of the governors to be communicated to the link D; for instance, a weighted lever without the link *h*, but having a roller at the end to bear up against the underside of the arm F, might be used for the purpose, or the weighted lever K and link *h*, might be dispensed with, and the link E suspended from a weighted chain passing over a pulley to be acted on by the governor, in fact, other devices will suggest themselves to expert mechanics.

If the combination of the parts D,E,G, and F operating in conjunction with each other as described is new, I can see no reason why the Office should insist upon the striking out of the words "*or their equivalents*," as applied to the other parts K and *h*, and thus put the applicant in possession of a patent which the public would declare to be weak, which would be liable to attacks from unscrupulous men, and would be more of an injury to the patentee than a benefit.

Although my remarks have been extended beyond the usual limits, and although my views have been expressed in perhaps unnecessarily urgent terms, I am satisfied that the Office will not consider me to be actuated by any spirit of fault-finding, or the desire to assume a dictatorial position.

Whatever errors there may be in the above views on an important subject, and the Office may be able to convince me that I have erred in some respects, they have not been coined for an especial purpose, but have been suggested by experience and by intimate knowledge of the opinions and wants of inventors.

In conclusion let me direct the attention of the Examiner in charge to the recent decision by the Board of Examiners in Chief, in the case of J. S. Mason's application for a patent for a design for a label, a decision which I am satisfied was prepared after the most careful consideration by the gentlemen composing that board.

I am, very respectfully, yours,

Henry Howson, Attorney for R. Heys.

English vs. *Swedish Iron.*

From the London Builder, No. 1088.

Experiments of an important nature have just been made at the fortress of Carlberg, in Sweden, upon the respective merits of armorplate made in England, France, and Sweden. Messrs. John Brown & Co., of Sheffield, sent two plates, one 12 feet by 2 feet 6 inches, and one 6 feet by 3 feet 8 inches. Messrs. Petin, Gaudit & Co., of Lyons, sent two plates, each of 7 feet 6 inches by 3 feet 3 inches. The Montala Iron-works Company, of Sweden, sent two plates of 12 feet by 2 feet 6 inches, and one 6 feet by 3 feet 8 inches. All the plates were 4½ inches in thickness, and then bolted to a teak target backed with iron plating, and supported by a massive stone pier. The two upper plates in the target were the French, and each was secured by 11 bolts. The next plate below was the longest, Swedish, and this was secured by 29 bolts. Below this was a tier of two short plates, one Swedish and one English, each secured by 24 bolts, and the lowest plate was a long English, secured, like the Swedish, by 29 bolts. Each plate received six shots from the ordinary 68-pounder naval gun. The French and Swedish plates broke to pieces, and the English plates remained uninjured and free from cracks. The shots used were of Swedish iron, and exhibited great toughness as compared with the shots used in the English service—the core or centre of the shot, after striking, being of double the weight of the core of the English shot.

Army and Navy Gazette.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, March 17, 1864.

William Sellers, President, in the chair.
John H. Towne, } Vice Presidents. }
Fairman Rogers, } } present.
Robert Briggs, Corresponding Secretary. }
John F. Frazer, Treasurer.
Washington Jones, Recording Secretary.

The minutes of the last meeting were read and approved.

Letters were read from the Royal Geographical Society, London; l'Academie Royale des Sciences de Lisbonne, Portugal; Capt. J. M. Gillis, U.S.N., United States Naval Observatory, Washington, D. C.

Donations to the Library were received from the Royal Astronomical Society, the Royal Geographical Society, the Society of Arts, and the Chemical Society of London; l'Ecole des Mines, Paris, et la Société Industrielle de Mulhouse, France; l'Académie Royal des Sciences de Lisbonne, Portugal; the Canadian Institute of Toronto, and the Natural History Society, Montreal, Canada; the U.S. Naval Observatory, the Hon. Wm. D. Kelley, and F. Emmerick, Esq., Washington, D.C.; the Board of Water Commissioners, Jersey City, N.J.; the Young Men's Association, Milwaukee, Wisconsin; and from Dr. T. S. Kirkbride, Wm. J. Mullen, Esq., John P. Whipple, Esq ,U.S.N., Prof. John F. Frazer, and A. M. Spangler, Esq., Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's report for February was read.

The Actuary reported that the following Standing Committees have organized by electing their chairmen and appointing the time for holding their stated meetings.

Committee.	*Chairman.*	*Time of Meeting.*	
On Library,	George Harding,	1st Tuesday	Evening,
" Exhibitions,	T. Morris Perot,	1st Wednesday	"
" Arts and Manufactures,	John H. Cooper,	1st Friday	"
" Cabinet of Minerals,	Elias Wildman,	2d Monday	"
" Cabinet of Models,	William B. Bement,	2d Monday	"
" Science and the Arts,	John C. Cresson,	2d Thursday	"
" Meetings,	Henry Howson,	3d Thursday	"

The Special Committee on the revision of the By-Laws, presented their report, which was read and accepted.

The Special Committee on the Weights, Measures, and Coinage of the United States reported their progress, and were continued.

A Committee appointed by the Board of Managers, reported a draft of the amendments proposed to be made in the Charter of the Institute, which was discussed and several amendments proposed; when a committee was appointed to prepare an amended Charter of the Institute, in accordance with the memorial just adopted.

On motion, it was

Resolved, That the Institute do hereby approve of, and direct the President to affix and attest the seal of this corporation to the petitions which have just been read, and to transmit them to the Senate and House of Representatives, respectively, in such manner as he shall deem proper.

Resignations of membership in the Institute by six gentlemen, were read and accepted.

Candidates for membership in the Institute (10) were proposed, and the candidates proposed at the last meeting (6) were duly elected.

The President read a letter from the Hon. Gideon Welles, Secretary of the U.S.N., requesting the Institute to appoint a committee of three members to act jointly with like committees to be appointed by the Navy Department and by the National Academy of Sciences, to conduct, witness, and report upon, experiments which may be agreed upon by the commission on the expansion of steam.

On motion, the President was authorized to appoint the committee to act on the joint committee, as requested by the Navy Department, when he named the following gentlemen, who were approved:

Committee.—Messrs. John H. Towne, J. Vaughan Merrick, and Richard A. Tilghman.

Mr. Chase continued the abstract of his papers on "Tides of Rotation," and made some remarks respecting the alleged connexion between the variable rate of the earth's rotation and the mean temperature of given parts of its surface.

Mr. Colburn's inquiry into the nature of heat suggests some interesting speculations concerning other efforts of rotation than those that can be measured by the barometer. Recognising the impossibility that the sun should warm the whole solar system, as a simply incandescent body,—the improbability that its heat should result from continuous combustion, and the probable approximate uniformity of temperature in the upper regions of the atmosphere, in summer and in winter, by day and by night, Mr. Colburn looks for the principal sources of heat to the earth itself. He supposes, 1, that the solar attraction tends to draw into closer proximity the particles of air on the heated side, and to separate them on the night-side of the earth, thus producing heat of compression, and cold of expansion. 2, that the change of eastward velocity from 67,000 miles per hour at midnight, to 69,000 miles at noon (*sic*), necessarily produces a conversion of motion into heat, and of heat into motion. And, 3, that if the earth is moving in a resisting medium, by which it is so retarded that it approaches the sun at the rate of 1,000.000 miles in 3,000,000 years, its "lift" involves the annual abstraction of heat-force equivalent to 752,665,108,390,000 horse-power!

The third hypothesis has been often broached; the indications of a resisting ether, which, as has been seen, are furnished by the hourly barometric means, may, perhaps, yield the data for its final verification or rejection. The supposed separating effect of the sun's action in the most remote portions of the atmosphere, is so problematical, that it seems hardly deserving of any consideration, and even if it existed, it is difficult to understand how it could produce a difference of more than a fraction of a degree in the range of the thermometer. The alternate acceleration and retardation of orbital velocity, can produce no *accumulation* of heat to supply any loss that may arise from radiation into space, but it must modify the *distribution* of heat throughout the day in a manner that may be readily calculated. The available data are not sufficient to furnish us with complete results, but they give curious approximations that seem to open a wide field for profitable investigation.

"Sir John Herschell finds the direct heating effect of a vertical sun at the sea level, to be competent to melt ·00754 of an inch of ice per minute; while according to M. Pouillet, the quantity is ·00703 of an inch."* Taking the mean of these two estimates (·00728 in.), multiplying by the latent heat of water, (142·6° F.), and dividing by the number of cubic ins. in 1 lb. of water (28), we obtain $\frac{\cdot00728\times142\cdot6}{28}=$ ·037076 units of heat received per minute on each square inch of the earth's surface that is exposed to a vertical sun. The weight of the aerial column being 15 lbs., and its ratio of specific heat ·25, the maximum effect of the direct solar rays is sufficient to heat the whole atmosphere $\frac{\cdot0371}{15\times\cdot25}$ per minute, or 7·12° F. in 12 hours.

Now, in consequence of the earth's rotation, the difference of atmospheric "lift" between noon and midnight, is 182,336 feet per minute. The average difference for the twelve hours, is one-half as great. "Rapid rotation, without friction or resistance, cannot in itself alone be regarded as a cause of light and heat;"† but we have found in our barometric investigations, that the ratio of the half-daily velocity of rotation to that which would be conferred by twelve hours action of terrestrial gravity, is ·00109, which may be regarded as the modulus of heat-producing resistance. If we multiply the average difference of lift by the weight of the atmosphere, and by the effective resistance, dividing the product by the ratio of specific atmospheric heat, and the number of foot-pounds raised by a unit of heat, we obtain $\frac{91168\times15\times\cdot00109}{770\times\cdot25}=7\cdot74°$ F. as the amount of heat communicated to the air by rotation between midnight and noon, and abstracted between noon and midnight.

The theoretical barometric lift is, as we have seen, ·00219 of the entire weight of the atmosphere. Estimating the height of the aerial column when reduced to uniform surface density, at 24,000 feet, the heat-producing disturbance that is indicated by the barometer, is represented by a lift of 15 lbs. on each square inch to a height of ·00219×24000 feet. The quarter-daily disturbance from this cause is, therefore, $\frac{24000\times15\times\cdot00219}{770\times\cdot25}=4\cdot1°$ F.

It is more than likely that each of these results will require important modifications when the entire influence of the several conditions of the problem is better understood. I have thought it proper to present them in their present crudity, in order to show the true points of departure, and to prepare the way for some further considerations.

Whatever other heat-disturbing causes there may be, there can be little doubt that the three we have just been considering are the most important. Dividing the astronomical day into four quadrants, and representing the solar effect by S, rotative by R, and barometric by B, it will readily be seen that the several positive and negative influences must be distributed as follows:

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* Tyndal, Heat considered as a Mode of Motion. N. Y. edit. p. 431.

† Dr. J. R. Mayer.

	S.	R.	B.
From 0h. to 6h.	+	—	—
" 6h. to 12h.	—	—	+
" 12h. to 18h.	—	+	—
" 18h. to 0h.	+	+	+

The tables of average temperature at any given place would therefore furnish us with four equations for determining the value of each of the disturbing elements, provided those that are unknown were so insignificant as to be safely neglected. The effects of these unknown disturbances are confined within certain limits that can be pretty satisfactorily determined.

Our discussion of the barometric fluctuations demonstrated a tendency of inertia to retard the effects of rotation, so that the mean daily altitudes are found nearer to 1h., 7h., 13h., and 19h., than to 0h., 6h., 12h., and 18h. A like tendency is discernible in the thermometer.

There are three, and only three, quadrantal divisions of the day, commencing respectively at 0h., at 1h., and at 2h., for which we could obtain approximate positive values of S., R., and B. The maximum solar effect is deduced from the first, and the minimum from the third of these divisions; while the maximum rotative and barometric effects are exhibited in the third, and the minimum in the first division.

The nearest average temperatures are found in the third division, as is shown below.

Average of Temperature at 2h., 8h., 14h., and 20h., and of the entire day.

STATION.	Mean of the four hours.	Daily mean.
At Girard College, . .	52°·1	52°·1
At St. Helena, . .	61°·65	61°·69

The following table presents all the co-ordinate positive values of S., R., and B., that can be obtained from the Girard College and St. Helena means.

STATION.	DIVIDING AT								
	0,6,12,18 h.			1,7,13,19 h.			2,8,14,20 h.		
	S.	R.	B.	S.	R.	B.	S.	R.	B.
	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.	Per ct.
Girard College,	45·92	41·32	12·76	31·3	49·5	19·2	13·8	63·2	23·
St. Helena, .	25·97	42·96	31·07	15·8	46·7	37·5	5·6	56·6	37·8

The percentages of the calculated values correspond very nearly with the means of the earliest Girard College and St. Helena values.

	Calculated.	Percentage.	Mean Percentage.	Limits.	
S, .	7°·12	37·6	35·95	5·6	45·92
R, .	7°·74	40·8	42·14	40·8	63·2
B, .	4°·1	21·6	21 91	12·76	37·8

It may be inferred from this comparison that the rotation element of daily heat is least affected, and the solar element most affected by extraneous causes (of which moisture is probably the chief); that the first division gives the best, and the third division the poorest results; that the proportion of thermometric variation which is attributable to rotation is between ·4 and ·5 of the average total daily variation, and that the most difficult element to determine satisfactorily is S., which is modified by many local disturbing influences, such as the nature of the soil, amount of vapor, clouds, altitude of the sun, &c. &c.

Prof. A. L. Fleury read the following papers:

I. *On Carbonic Acid and Artificial Spring Waters.*

It is only a few years since the value of Aërated Waters—waters impregnated with Carbonic Acid which holds many otherwise insoluble substances in perfect solution—has become better appreciated, and as spring and warm weather will soon begin to make their appearance, I beg your attention for a few moments to a new improvement in the so-called Transportable Fountains.

Everything that tends to preserve and improve our health, deserves, or better said, should, by right, be entitled to our consideration. For the last year, Messrs. Schultz & Walker, 133 4th Avenue, in New York, supplied the public with a superior article of Carbonic Acid, and Artificial Spring Waters, such as Selters, Vichy, Kissingen, Carlsbad, Püllna, Iron Water, Citrate of Magnesia, and others in the fountains here exhibited, at prices varying from 15 to 25 cents for contents of bottle. These fountains have been imported from France at a great cost; every one is tried with a pressure of 15 atmospheres before it is allowed to leave the factory. Mr. Carl Schultz, of the U. S. Assay Office in New York, remarked to me, that notwithstanding his numerous inquiries he has not yet been able to have these bottles made in this country, and he is very desirous to reduce the price of the fountain, which now with the high price of gold is over $2 per fountain. I have here a few pieces of the glass of which the fountains are composed. Carbonic acid has recently found a new application for the relief of *gout*, and as a soothing medium for the pain produced by exterior inflammation. The greatest caution is necessary, however, in its use, because it must be remembered, when *inhaled*, this gas is poisonous.

This carbonic acid fountain, by being used the upper side down, when the gas alone without the liquid escapes, can be used wherever a moderate supply of some 30 or 40 gallons of the gas is required.

Others gases, as for instance, oxygen, chlorine, &c., might be preserved compressed in these fountains.

II. *On Solution of Iodine and Pyrophosphate of Iron.*

Mr. E. Fougera, Pharmaceutist of the Long Island College Hospital in New York, has forwarded to me some new preparations. At our last meeting I had not yet received the full details of these valuable preparations. They have since been given to me, and I beg to communicate them to the members of the Institute.

For his numerous experiments to dissolve iodine in syrup, he made the following observations:

1. The spontaneous decomposition of iodide of iron dissolved in syrup, is arrested at a certain point, and goes no further, even when exposed for months in a cellar, covered simply by a cap of paper.

2. This slightly decomposed syrup, or only colored with a small quantity of iodine, added on purpose, becomes perfectly white after a long exposure to the rays of the sun, or a moderate heat. But placed in the dark, its amber-color re-appears.

The first fact overthrows the existing theory of the decomposition of the syrup of iodide of iron, explained by the formation of a protoxide of iron and of hydroiodic acid, by means of the decomposition of water into its elements, and the subsequent transformation of the protoxide into sesquioxide of iron by the oxygen of the air. It is evident that, if the decomposition of the water and of the iodide of iron takes place in this way, it would certainly continue until all the iodide of iron is decomposed, a circumstance that does not take place.

To explain the second fact, I asked myself what became of the free iodine; evidently it could not combine with the iodide of iron; further we know that scarcely $\frac{7}{1000}$ of iodine will dissolve in water, to form, as some chemists maintain, iodic and hydroiodic acids. It remained only to ascertain what action free iodine has on sugar.

To solve this question, I made numerous experiments with iodine and syrup of sugar, and found immediately that under a slight and prolonged heat, the pulverized metalloid added to the syrup is therein dissolved in all proportions.

Up to 2 grammes of iodine added to 30 grammes of syrup, I obtained a colorless solution. The larger the proportion of the iodine is, the more care is necessary to take the syrup from the bath so soon as it is white. If further exposed to heat, the syrup becomes dark, the sugar being converted into caramel; this caramel in its place, becomes also decomposed under generation of carbonic acid, leaving a light black and spongy residuum, which is partly soluble in water and alcohol. Treated with hydrochloric acid, potassa, &c., this substance comports itself like ulmine and ulmic acid. To construct the operation to the entire decomposition of sugar, the necessary precautions have to be taken to evade the fracture of the glass by the expansion of the gas, which forms itself in large quantity, especially when the heat is somewhat elevated.

This iodated syrup has sometimes the odor of fruit; it is acid, unalterable in the air, decomposed at 100° Centigrade, and contains much glycose. Treated by re-agents it comports itself like iodides in general.

Here are the facts; it remains to give the theory. Does the iodine combine with the sugar, $C_{12} H_{11} O_{11} F$, or with the glycose, $C_{12} H_{14} O_{14} J$, as it does with starch, $C_{12} H_{10} O_{10}$, to form iodides? Or does the iodine in presence of sugar, which would act by catalysis, decompose the water and form iodic and hydroiodic acids? Which acids are formed, and are they formed at the expense of the water, or the sugar? Bromide acts in the same way, only more rapidly.

Chlorine acted on syrup still more rapidly than bromine. To water that had recently been saturated with chlorine under cold, sugar was added, and gently heated. In less than half an hour, the chlorine had disappeared and the liquid was slightly acid. It is probable that the chlorine was transformed into hydrochloric acid.

The iodated oil contains 8 grains of iodine, 1 grain of bromine, and 1 grain of phosphorus to the pint. It is prepared as follows: In 4 oz. of iodated syrup are dissolved (in the cold) in a mortar 8 grains of iodine; one pint of oil is added, and the whole well agitated. After the liquids have separated the syrup is decanted. One grain of bromine is dissolved in a little bromated syrup, and added to the above-mentioned pint of iodated syrup, mixed, and the syrup after standing decanted. As to the phosphorus, it is dissolved in a little oil at a gentle heat, and then mixed with the above-named preparation. The whole is finally filtered.

These oils have received the testimonials of the highest authorities, and Mr. Fougera deserves the thanks of the chemist as well as of us all, for his communications. This *Pyrophosphate of Iron*, ably elucidated by Dr. O. N. Chapman, of this city, as well as other preparations, deserve a wide-spread reputation, being prepared in the most careful and reliable manner.

Dr. J. A. Thompson, of Auburn, New York, presented his patent Photograph Cabinet, for the exhibit and preservation of Photographs and other Pictures. This cabinet is so arranged that the pictures are readily brought into view and exhibited beneath a plain transparent glass, producing the full softening effect of a frame. They are excluded from the injurious action of air, light, moisture, dust, and handling, which are so necessary in the preservation of these delicate pictures, that his cabinet cannot fail to take its place among the useful and ornamental inventions.

U. B. Vidal's improved Coal Oil Lamp was exhibited. The wick in this lamp is brought from the reservoir through a wick-tube of the ordinary width, but near the top is spread out to double that width, so as to give a flame of great size and brilliancy.

Mr. John May exhibited his improved Vertical Submerged Paddle Wheels for canal boats. The paddles are contained within a vertical revolving drum, from which they can be projected so as to strike the water on any side of the drum desired.

Philip Griffith's Patented Grate Bars, were also exhibited. These bars have dove-tailed projections and recesses at the sides, so that while they can be secured firmly together, one or more can readily be removed by sliding the bar back so as to disengage its dove-tailed projections from the recesses in the bar next to it.

Mr. H. Howson exhibited the following articles:

Robert Ross' improved Oil Cup, which has a central reservoir, and hollow spindle surmounted by a cup, so arranged in respect to a valve at the lower part of the reservoir, that on turning the spindle in one direction the oil will flow from the cup into the reservoir, and on reversing the motion of the spindle, from the reservoir on to the part to be lubricated.

Shaw's Tobacco Pipe, with a reservoir beneath the bowl for the reception of moisture from the mouth.

S. Winner's Self-instructing Scale for Pianos. This consists of a number of blocks, so formed that they may be readily placed against the key-board of a piano; resting on the keys without interfering with the action of the latter. On the blocks are painted lines representing a musical staff, with notes, each of which represents the key directly beneath it, the letter or name of the note being painted directly below the latter, so that the scholar as he strikes a key, may see at once its name and the position on the staff of the note designating it.

W. Starkey's improved Lamp Chimney. The lower part of the chimney is of glass, which is surmounted by a metallic rim, to which is secured at two or three points, a cone-shaped funnel. The connexion between the latter and the rim is so slight, that the heat imparted to the funnel by the hot gases arising from the flame cannot be transmitted to the rim or the glass, which is consequently prevented from becoming suddenly or unequally heated.

Alden's "Chuck" for Lathes, from each of the "jaws" in which a pin projects into a curved radiatory groove in the face of a disk; the turning of the disk, in one direction or the other, opening or closing the jaws.

Specimens of Fire-Arms, manufactured by C. Sharps. Also, Sharp's Self-priming Pistol, manufactured by S. Butterfield.

C. Hutchinson's Tobacco Pipe, in which a moistened sponge is arranged between the bowl and the opening leading to the stem, to cool the smoke before passing to the mouth.

Specimens of Japanese Paper, which both in appearance and texture bears a strong resemblance to morocco.

Messrs. Altemus & Co.'s improved hinged-back Album, in which each leaf of the Album is secured by means of a leather hinge to the one next to it.

A Comparison of some of the Meteorological Phenomena of MARCH, 1864, *with those of* MARCH, 1863, *and of the same month for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	March, 1864.	March, 1863.	March. for 13 years.
Thermometer—Highest—degree, .	59·00°	63·00°	78·50°
" " date, .	13th.	25th.	3d, 1861.
" Warmest day—Mean,	48·50	56·67	66·00
" " " date,	13th.	25th.	3d, 1861.
" Lowest—degree, .	20·00	15·00	4·00
" " date, .	22d.	5th.	10th, 1856.
" Coldest day—Mean,	27·33	24·17	11·59
" " " date, .	22d.	15th.	10th, 1856.
" Mean daily oscillation,	13·55	13·81	14·78
" " " range, .	5·11	6·47	6·08
" Means at 7 A. M., .	34·35	32·29	35·39
" " 2 P. M., .	45·07	40·94	46·54
" " 9 P. M., .	39·03	35·82	40·09
" " for the month,	39·48	36·35	40·67
Barometer—Highest—Inches, .	30·131 in.	30·384 in.	30·522 in.
" " date, . .	3d.	21st.	3d, 1852.
" Greatest mean daily press.	30·090	30·311	30·445
" " date, . .	3d.	20th.	11th, 1852.
" Lowest—Inches, .	29·184	29·422	29·158
" " date, . .	30th.	25th.	17th, 1854.
" Least mean daily press.,	29·246	29·497	29·246
" " date, . .	30th.	25th.	30th, 1864.
" Mean daily range, .	0·176	0·214	0·194
" Means at 7 A. M., .	29·723	29·882	29·841
" " 2 P. M., .	29·656	29·829	29·784
" " 9 P. M., .	29·699	29·880	29·821
" " for the month, .	29·693	29·864	29·815
Force of Vapor—Greatest—Inches,	0·361 in.	0·445 in.	0·549 in.
" " " date, .	5th.	25th.	18th, 1859.
" " Least—Inches, .	·064	·050	·023
" " " date, .	20th.	15th.	5th, 1858.
" " Means at 7 A. M.,	·146	·147	·161
" " " 2 P. M.,	·149	·151	·176
" " " 9 P. M.,	·160	·153	·178
" " " for the month,	·152	·159	·172
Relative Humidity—Greatest—per ct.	95 per ct.	94 per ct.	100 per ct.
" " " date,	31st.	11th.	Often.
" " Least—per ct.,	18·0	26·	16·0
" " " date, .	9th.	27th.	31st, 1860.
" " Means at 7 A.M.,	71·5	75·4	73·6
" " " 2 P.M.,	50·4	56·4	53·0
" " " 9 P.M.,	65·8	70·2	67·7
" " "for the month	62·6	67·4	64·8
Clouds—Number of clear days,* .	10	4	9·9
" " cloudy days, .	21	27	21·1
" Means of sky cov'd at 7 A.M.,	61·0 per ct.	61·8 per ct.	59·9 per ct.
" " " " 2 P.M.,	63·5	72·3	61·1
" " " " 9 P.M.,	50·3	55·2	45·3
" " " for the month,	58·3	65·2	55·4
Rain and melted Snow—Amount, .	5·243 in.	6·379 in.	3·241 in.
No. of days on which Rain or Snow fell,	10	16	10·7
Prevailing Winds—Times in 1000,	N.46°7′W.·221	N37°45′W·235	N69°15′W·297

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* Less than one-third covered at the hours of observation.

JOURNAL

OF

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OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

MAY, 1864.

CIVIL ENGINEERING.

For the Journal of the Franklin Institute.

The Hydrostatic Trough.

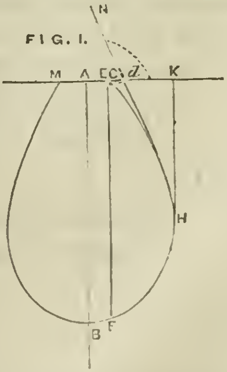
By De Volson Wood, Prof. C. E. University of Michigan.

Problem.—If a perfectly flexible and inextensible trough have its edges firmly fixed in a horizontal plane, and filled with a heavy fluid, it is required to find the equation of a transverse section.

Or, the problem may be stated in the following way:

If a perfectly flexible and inextensible arc, MBC, Fig. 1, be fixed at its ends, M and C, and filled with a heavy fluid; it is required to find its form under the pressure of the fluid. This I call "A Hydrostatic Trough," in distinction from the "Hydrostatic Arch" of Von Villarceaux.

FIG. 1.



The problem which I have proposed is that of a normally pressed arc. But few problems of such curves have, to my knowledge, ever been solved. The first one is very simple, being that of a ring placed horizontally under a fluid, in which case the curve is a circle; the second is the "Hydrostatic Arch" of Von Villarceaux, in which the arch is placed under a fluid so that the crown will be at a finite distance from the surface of the fluid. The third was investigated for a suspension bridge, by Mr. Robinson, and is published in the September Number of this Journal for 1863. In the following article I shall investigate the fourth one.

I will first investigate the general equations applicable to normally pressed arcs.

Let $f(x,y,z,)$ be the equation of the curve referred to rectangular co-ordinates,

T, the tension at any point,

$\text{X},\text{Y},\text{Z}$, the components of the impressed forces on a unit of length of the curve,

ds an element of the arc.

Then, $\text{X}ds$, $\text{Y}ds$, $\text{Z}ds$, are the components on an element of length.

$\text{T}\dfrac{dx}{ds}$ is the component of T on the axis of x, and

$d\left(\text{T}\dfrac{dx}{ds}\right)$ is the component of the tension on ds due to the impressed forces on the element, consequently equal $\text{X}ds$; similarly for the others.

$$\therefore \quad \left.\begin{aligned} d\left(\text{T}\frac{dx}{ds}\right) &= \text{X}ds \\ d\left(\text{T}\frac{dy}{ds}\right) &= \text{Y}ds \\ d\left(\text{T}\frac{dz}{ds}\right) &= \text{Z}ds \end{aligned}\right\} \qquad . \qquad . \qquad (1)$$

These equations are applicable to any curve in which the forces are continuous functions of the co-ordinates. But in a normally pressed arc T *is constant;* for it cannot vary unless there be a tangential component of the applied forces; but there can be no tangential component when they are all normal.

Observing this, squaring and adding, and we have

$$\text{T}^2\left[\left(d\frac{dx}{ds}\right)^2+\left(d\frac{dy}{ds}\right)^2+\left(d\frac{dz}{ds}\right)^2\right]=(\text{X}^2+\text{Y}^2+\text{Z}^2)\,ds^2 \quad . \quad (2)$$

Let

ρ = radius of curvature at point, $(x,y,z,)$

P = applied force on a unit of length, and equation (2) becomes

$$\text{T}^2\rho^2\,ds^2=\text{P}^2\,ds^2;$$

$$\therefore \text{P}=\frac{\text{T}}{\rho}. \qquad . \qquad . \qquad (3)$$

Hence, the applied force on each unit of length will be inversely as the radius of curvature.

In Fig. 1, let

$D = AB =$ total depth of the trough,
$\rho_0 =$ the radius of curvature at the lowest point,
$i =$ angle Ndk which the tangent at any point makes with the axis x,
$i_0 =$ the value of i at the superior element,
$w =$ weight of unit of volume of the fluid.

Take the origin at A, the middle point of the span; x horizontal, and y vertical.

Since the pressure of a fluid is proportional to its depth, we have

$P = wy$; nence from (3) we have
$T = wy\rho$ for any point, and
$T = wD\rho_0$ for the lowest point.

$$\therefore \quad wy\rho = wD\rho_0. \qquad (4)$$

$$\therefore \quad \text{or } \rho = \frac{D\rho_0}{y}, \qquad (5)$$

Hence the radius of curvature at any point is inversely as the distance of the point below the surface of the fluid; hence at the upper end it is infinite.

The second member of (4) is constant; hence it is the equation of an hyperbola referred to its asymptotes, in which ρ is the abscissa and y the corresponding ordinate.

Substituting the value of the radius of curvature in (5), and we have

$$\pm\left(1+\frac{dy^2}{dx^2}\right)^{-\frac{3}{2}}\frac{d^2y}{dx^2} = \frac{y}{D\rho_0}.$$

The first integral is

$$\mp\left(1+\frac{dy^2}{dx^2}\right)^{-\frac{1}{2}} = \frac{y^2}{2D\rho_0} + C \qquad (6)$$

In the second integral I find that the *minus* sign before the parenthesis gives an imaginary result; hence we use the plus sign only.

Observing that $\frac{dy}{dx} = 0$ for $y = D$, and (6) gives

$$C = 1 - \frac{D}{2\rho_0} = \frac{2\rho_0 - D}{2\rho_0}.$$

$$\therefore \left(1+\frac{dy^2}{dx^2}\right)^{-\frac{1}{2}} = \frac{y^2}{2D\rho_0} + \frac{2\rho_0 - D}{2\rho_0}. \qquad (7)$$

For $y = 0$, $i = i_0$ and $\left(1+\frac{dy^2}{dx^2}\right)^{-\frac{1}{2}} = \cos. i_0$.

$$\therefore \cos. i_0 = \frac{2\rho_0 - D}{2\rho_0} = 1 - \frac{D}{2\rho_0}. \qquad (8)$$

$$\therefore \rho_0 = \frac{\frac{1}{2}D}{1 - \cos. i_0} = \frac{D}{4\sin^2\frac{1}{2}i_0}. \qquad (9)$$

$$\begin{aligned} \text{Hence if } i_0 &= 0, & \rho_0 &= \infty \\ i_0 &= 90^\circ, & \rho_0 &= \tfrac{1}{2}\,\mathrm{D} \\ i_0 &= 180^\circ, & \rho_0 &= \tfrac{1}{4}\,\mathrm{D} \\ i_0 &> 90^\circ, & \rho_0 &< \tfrac{1}{2}\,\mathrm{D} \\ i_0 &< 90^\circ, & \rho_0 &> \tfrac{1}{2}\,\mathrm{D} \end{aligned}$$

$\rho_0 = \frac{1}{4}\,\mathrm{D}$ is the limit of ρ_0 in the direction of its smallest value, but a limit it can never reach unless T is infinite; for the vertical component of T at the upper end must sustain one-half the fluid; or

$$w \int_0 x\, dy = \mathrm{T} \sin.\, i_0. \qquad . \qquad . \qquad . \qquad (10)$$

but for $i_0 = 180^\circ$, $\sin.\, i_0 = 0$ $\therefore$ T must be infinite if the first number is finite.

Some of the preceding expressions may be deduced in a more elementary way: thus, the total horizontal pressure of the fluid must equal the tension at the lowest point, together with the horizontal component at the upper end; or more generally, the horizontal components in one direction must equal those in the opposite direction. The horizontal pressure of the fluid is $\frac{1}{2}\, w\, \mathrm{D}^2$: the horizontal component of the tension at the upper end is $\mathrm{T} \cos.\, i_0$, and is negative when the angle is obtuse. We have then

$$\tfrac{1}{2}\, w\mathrm{D}^2 = \mathrm{T} - \mathrm{T} \cos.\, i_0.$$

$$\therefore \mathrm{T} = \frac{\frac{1}{2}\, w\, \mathrm{D}^2}{1 - \cos.\, i_0}. \qquad . \qquad . \qquad . \qquad (11)$$

Let $y_1 = \mathrm{KH} =$ the vertical tangent ordinate.

The horizontal pressure between y_1 and D equals the tension at the lowest point.

$$\therefore \tfrac{1}{2}\, w\, (\mathrm{D}^2 - y_1^2) = \mathrm{T} = w\mathrm{D}\rho_0. \qquad . \qquad . \qquad (12)$$

$$\therefore y_1 = \sqrt{\mathrm{D}^2 - 2\,\mathrm{D}\,\rho_0}. \qquad . \qquad . \qquad . \qquad (13)$$

Eliminating between (11), (12), and (13), and we find,

$$\rho_0 = \frac{\frac{1}{2}\mathrm{D}}{1 - \cos.\, i_0} \text{ which is the same as (9).}$$

If $i_0 = 90^\circ$ Equation (11) gives $\mathrm{T} = \frac{1}{2}\, w\mathrm{D}^2$
and " (9) " $\rho_0 = \frac{1}{2}\,\mathrm{D}$, and this
in " (13) " $y_1 = 0$, and this
in " (12) " $\mathrm{T} = \frac{1}{2}\, w\mathrm{D}^2$ as before.

If i_0 is less than 90°, we have

$$\tfrac{1}{2}\, w\, \mathrm{D}^2 + \mathrm{T} \cos.\, i_0 = \mathrm{T}.$$

$$\therefore \mathrm{T} = \frac{\frac{1}{2}\, w\, \mathrm{D}^2}{1 - \cos.\, i_0} \text{ which is the same as (11).}$$

Now returning the differential expression we observe that (6) gives

$$(dx^2 + dy^2)^{\frac{1}{2}} = ds = \frac{2\,\rho_0\,\mathrm{D}dy}{\sqrt{4\,\mathrm{D}^2\rho^{02} - (2\,\mathrm{D}\rho_0\,\mathrm{C} + y^2)^2}} \qquad (14)$$

$$\text{or} \quad dx = \frac{2\,\mathrm{D}\,\rho_0\,\mathrm{C} + y^2}{\sqrt{4\,\mathrm{D}^2\,\rho_0^2 - (2\,\mathrm{D}\,\rho_0\,\mathrm{C} + y^2)^2}}\,dy. \qquad . \qquad (15)$$

To find where x is a maximum, make $\frac{ds}{dy}=1$ in (14) or $\frac{dx}{dy}=0$ in (12); in either case we find

$$y=y_1=\sqrt{\mathrm{D}(-2\rho_0+\mathrm{D},)}$$ which is the same as (13).

Equations (14) and (15) may be integrated by means of elliptic functions. Substitute the value of c and make *

$$c^2=\frac{\mathrm{D}}{4\rho_0}=\sin^2.\,\theta \therefore \sin.\,\theta=\tfrac{1}{2}\sqrt{\frac{\mathrm{D}}{\rho_0}}. \qquad (16)$$

$$y=\mathrm{D}\cos.\,\varphi \therefore dy=-\mathrm{D}\sin.\,\varphi d\varphi. \qquad (17)$$

These in (14) give

$$s=\int\frac{-2\,\rho_0\,\mathrm{D}^2\sin.\,\varphi d\varphi}{\sqrt{\mathrm{D}^2\rho_0{}^2-(2\,\mathrm{D}\,\rho_0-\mathrm{D}^2+\mathrm{D}^2\cos.^2\varphi)^2}}$$

Make $\cos.^2\varphi=1-\sin.^2\varphi$, develop and reduce, and we find

$$s=\int\frac{-2\,\rho_0\,\mathrm{D}^2\sin.\,\varphi d\varphi}{\sqrt{4\,\rho_0\,\mathrm{D}^3\,(1-\frac{\mathrm{D}}{4\rho_0}\sin.^2\varphi)\sin.^2\varphi}}$$

$$=\sqrt{\rho_0\,\mathrm{D}}\int_0^{\varphi}\frac{-d\,\varphi}{\sqrt{1-c^2\sin.^2\varphi}}. \qquad (18)$$

The substitutions in (15) will give

$$x=\int\frac{-[\mathrm{D}\,(2\,\rho_0-\mathrm{D})+\mathrm{D}\cos.^2\varphi]\,d\,\varphi}{2\sqrt{\rho_0\mathrm{D}}\sqrt{1-\frac{\mathrm{D}}{4\,\rho_0}\sin.^2\varphi}}$$

$$=-2\sqrt{\rho_0\,\mathrm{D}}\left[\frac{\frac{1}{2}-\frac{\mathrm{D}}{4\,\rho_0}\sin.^2\varphi}{\sqrt{1-\frac{\mathrm{D}}{4\rho_0}\sin.^2\varphi}}\right]d\,\varphi$$

By adding and subtracting 1 to the numerator, it may be put under the following form, viz:

$$x=-2\sqrt{\rho_0\,\mathrm{D}}\left[-\tfrac{1}{2}\int_0^{\phi}\frac{d\,\phi}{\sqrt{1-c^2\sin.^2\phi}}+\int_0^{\phi}(1-c^2\sin.^2\varphi)d\varphi\right] \qquad (19)$$

The complete integral of (18) is

$$s=\sqrt{\rho_0\,\mathrm{D}}\;\mathrm{F}_{1(c)}. \qquad (20)$$

The general integral of (19) is

$$x=2\sqrt{\rho_0\,\mathrm{D}}\left[-\tfrac{1}{2}\mathrm{F}_{(c,\,\phi)}+\mathrm{E}_{(c,\,\phi)}\right] \qquad (21)$$

in which we compute the arc from the lowest joint. I have changed the sign of x, for the curve is symmetrical with the axis y; and hence the positive value will equal the negative one.

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* See Legendre's Fonctions Elliptique, Chap. III.

For the half span AC, we have from (17) and (21,

$$y=0, \therefore \varphi = 90^\circ$$

$$x = x_\circ = 2\sqrt{\rho_\circ D}\left[-\tfrac{1}{2}F_{1(c,)} + E_{1(c,)}\right]. \qquad (22)$$

Equation (21) is the equation of the curve, and by means of it and (17) it may be constructed. We see that the *form* is independent of the weight of the fluid.

In problems of this kind the data would naturally be the length of arc and span, but with such data the problem can only be solved by successive approximations; for c, which is a function of D in (21), cannot be found explicitly. But if D and $\rho_\circ$ are known we may obtain a direct solution.

For example, let $\rho_\circ = \frac{16}{49}$D.

Then from (16) $c = \sin.\theta = \frac{7}{8} \therefore \theta = 61^\circ\ 3'$.

In the second volume of Legendre's *Fonctions Elliptique,* Table VIII, we find for $\theta = 61\frac{0\,1}{20}$ that $F_{1(c,)} = 2{\cdot}18566$.

Hence the total length is

$$2\,s = 2\sqrt{\frac{16}{49}D^2} \times 2{\cdot}18566 = 2{\cdot}52912\,D.$$

From the same table we find

$$E_1\left(61\tfrac{0\,1}{20}\right) = 1{\cdot}20106.$$

Then (20) gives $2x = 6{\cdot}2474\ D \qquad (23)$

for the total span at the upper end.

To construct the curve, we will call $\theta = 61^\circ$. From equations (21) and (17) and Table IX, we find

for $\varphi = 0^\circ$	$y = 1{\cdot}000\ D$	$x = 0{\cdot}000$
$\varphi = 20^\circ$	$y = 0{\cdot}940\ D$	$x = 0.190\ D$
$\varphi = 40^\circ$	$y = 0{\cdot}766\ D$	$x = 0{\cdot}325\ D$
$\varphi = 50^\circ$	$y = 0{\cdot}643\ D$	$x = 0{\cdot}354\ D$
$\varphi = 53^\circ$	$y = 0{\cdot}602\ D$	$x = 0{\cdot}357\ D$
$\varphi = 54^\circ$	$y = 0{\cdot}587\ D$	$x = 0{\cdot}361\ D$
$\varphi = 55^\circ$	$y = 0{\cdot}574\ D$	$x = 0{\cdot}360\ D$
$\varphi = 70^\circ$	$y = 0{\cdot}342\ D$	$x = 0{\cdot}308\ D$
$\varphi = 90^\circ$	$y = 0{\cdot}000\ D$	$x = 0{\cdot}125\ D$

By means of these values, I have constructed Fig. 1.

For the maximum abscissa we have from (13) $y_1 = \frac{1}{7}D\sqrt{17} = 0{\cdot}5890\ D$, which in (17) gives $\varphi = 53^\circ\ 54'$, which agrees well with the table above. This value of φ in (21) will give x.

For the inclination at the upper end we have from (8), $i_\circ = 111^\circ\ 18'$.

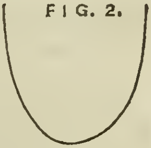
For the tension, (4) gives $T = \frac{16}{49}w\,D^2$.

For the total weight of the fluid, we have $2\,\mathrm{T}\sin. i_{\circ} = \frac{32}{49} w\,\mathrm{D}^2 \times 0{\cdot}93789$; hence the total area of the curve is $\frac{32}{49}\mathrm{D}^2 \times 0{\cdot}93789$.

Fig. 2 is the curve when the upper ends are vertical.

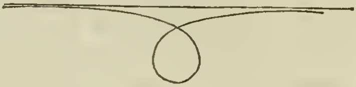
Suppose $\rho^{\circ} = \frac{1}{4}\mathrm{D}$; then we will have $\varphi = 90^{\circ}$, and this in (21) gives $x = -\infty$; hence the axis of x is an asymptote to the curve, as shown in Fig. 3.

FIG. 2.



To find ρ_0 when the upper ends terminate in the origin. For this x and y are zero, and (21) gives

FIG. 3.



$\frac{1}{2}\mathrm{F}_{1(c_,)} = \mathrm{E}_{1(c_,)}$ from which c must be found. Table VIII, in Legendre, gives $c = \sin. 65^{\circ}\ 18'$ nearly, and observing that $c = \frac{1}{2}\sqrt{\frac{\mathrm{D}}{\rho_0}}$ and we find

$$\rho_0 = 0{\cdot}303\,\mathrm{D} \text{ nearly.}$$

To find the evolute of the curve.

Let $x_,$ and $y_,$ be the co-ordinates of the evolute. Then we readily find

$$-x_{\imath} = -x + \rho \sin. i.$$
$$y_{\imath} = \quad y - \rho \cos. i.$$

which combined with the preceding equations so as to eliminate x, y, ρ, and i, will give the equation of the evolute.

The question naturally arises whether this principle is applicable to shells filled with a fluid, and if so it may give important information on the construction of jugs and bottles. Conceive a shell to be generated by the revolution of one of these curves about the axis of y: the shell generated will be of uniform thickness. Now, intersect this shell with a plane through the axis of y, and examine the pressure along this intersection. We at once see that the pressure is greatest at the lowest point, but the thickness is uniform and hence does not fulfill the conditions.

If we suppose that the total resisting surface in all horizontal intersecting planes in the same, we meet with a similar difficulty, for then the shell would be thinner (if in the form of a jug) below the middle of the depth than at the top, but the pressure would be greater at the latter than at the former point. In the case of bottles we must consider the strain in different planes; hence our investigation gives no light upon their proper construction.

In the early part of this article I intimated that but few curves of normally pressed arcs have been investigated; but it may be well to add that if a string be drawn upon (or around) a perfectly smooth curved surface the tension will be constant and the pressure normal;

but in this case the curve is known *à priori.* Such curves are called *Geodesic*, and their form depends upon that of the surface about which they are drawn.

The Actual State of the Works on the Mont Cenis Tunnel, and Description of the Machinery Employed. By THOMAS SOPWITH, JR.

From the Lond. Civ. Eng. and Arch. Jour., March, 1864.

This tunnel would form the completing link of the Victor Emmanuel Railway, and be the means of putting France and Italy in direct railway communication. The railway on the French side was already opened to St. Michael in Savoy, and on the Italian side to Susa in Piedmont. When the whole line was completed the mails and traffic with India might perhaps be advantageously transferred from Marseilles to some Italian port, as the Mediterranean Sea transit would thus be materially shortened.

During the last twenty years, many routes had been surveyed and recommended for crossing the great barrier of the Alps. Of these, that by the Mont Cenis was generally considered the most feasible; and that it was only a question, whether the mountain should be crossed by a series of inclines, or whether a tunnel should be made. In 1857, Messrs. Sommeiller, Grandis, and Grattoni, brought before public notice a new system of boring by machinery, instead of by hand labor. A government commission was appointed to report upon it, and to see if it could be applied to the boring of the tunnel under Mont Cenis. Their report was favorable, and M. Sommeiller and his partners were shortly afterwards charged with the execution of the work.

The ends only were available for attack, it being impossible, as was known from the first to sink shafts. It was feared that the ventilation would seriously retard, or altogether prevent, the completion of the tunnel: but this fear was uncalled for, as the artificial ventilation of collieries overcame greater natural difficulties, and the ventilating current passed through a longer distance, than could possibly be required in this tunnel. M. Sommeiller also proposed to use compressed air for driving the machinery, and calculated that on its escape, a volume of fresh air would be supplied adequate to the requirements of the workmen. The tunnel at the Modane, or French side, was of the following dimensions:—25 ft. 3½ ins. wide at the base, 26 ft. 2¾ ins. wide at the broadest part, and 24 ft. 7 ins. in height; the arch being a semicircle nearly. At Bardonnèche, the height was increased 11¾ inches. The exact length between the ends was 7·5932 miles. The present ends would not be the permanent entrances, as it was intended that a curved gallery should leave the tunnel at the north side, 415 yards from the end, and at the south side, 277 yards.

At Modane, the tunnel, was built entirely with stone; at Bardonnèche, for the greater part, the side walls only were of stone, and the remainder of brick. The Bardonnèche end was 434 feet higher than that at Modane. For one-half the length of the tunnel, therefore, from Modane to the middle, the gradient would be 1 in 45½; the other side being driven with only sufficient fall, 1 in 2000, to allow of the water escaping.

When the tunnel was complete, it was expected that there would be a constant current of air from the north to the south; the latter was not only the higher end, but the air was more rarified and exposed to the heat of the sun, whilst the entrance at Modane, was under the shade of the mountain.

The establishment consisted at each end, of machinery for compressing the air, workshops for making and repairing the machinery, offices, storehouses, residences for the engineers, and barracks for the workmen. At Modane, the entrance of the tunnel was 328 feet above the bottom of the valley, where the workshops were placed, with which there was a communication by means of an inclined plane, worked by a water balance.

Different systems of tunneling by machinery had been tried in England; amongst others one by Captain Penrice, R. E., in which it was intended to drive a gallery about 4½ feet diameter, and, by means of repeated blows from a heavy frame loaded with knives, to reduce the whole of the excavated materials to small chippings and dust. It seemed, however, to the author, that any system of tunneling must be deficient, which did not make so cheap and readily applicable a power as gunpowder available; and that by the trituration of the rock to such small particles as in Captain Penrice's system, a great amount of work was unnecessarily performed.

In M. Sommeiller's system, whilst machinery was employed for accelerating the progress usually made by hand labor, gunpowder was also available. He had succeeded in producing a compact machine, not weighing more than 6 cwt., which could pierce a common bore-hole, about 1¼ ins. diameter, and 3 feet deep, into a rock in twenty minutes, where two miners would have required two hours. Further, he had arranged a movable support capable of carrying eleven such machines, any one of which could be worked at almost any angle, and of allowing the free action of each, in a gallery 10 feet square. This support could be removed when it was necessary to explode the holes bored by the machines. The machine was of very ingenious construction. It consisted of two parts:—one, a cylinder for propelling the borer against the rock; the second, a rotary engine for working the valve of the striking cylinder, turning the borer on its axis at each successive stroke, and advancing, or retiring, the striking cylinder as occasion required. It gave 250 blows per minute. The effective pressure on the piston in striking was 216 lbs.; the length of the stroke was from 2 inches to 7½ inches. Although simplified as much as possible, the nature of the work the machines performed was so severe, that they were liable to frequent derangement, and a large stock was kept on hand. The cost of each machine was about £80. The compressed air was used at a pressure of five atmospheres above atmospheric pressure, and was conveyed to the "fore-head" of the advanced gallery by a pipe 7⅝ inches diameter. The advanced gallery was the only place where the machines were used; the enlarging of the tunnel to the full size, walling, &c., were performed by manual labor.

The system of working was to bore about eighty holes in the fore-head of the advanced gallery. The frame and machines were then withdrawn, and a set of men charged and fired the holes; afterwards replaced by another set to remove the déblais. The division of time amongst the different classes of labor was very variable. It might however, be averaged as

From 6 to 8 hours for the machinists,
" $1\frac{1}{2}$ " 2 " for charging and firing, and
" 3 " 5 " for removing the déblais.

Thus there were almost two complete shifts every twenty-four hours. An alignment was made about once in three months, from an observatory at each end. As yet no error had been detected.

Three or four large holes, each about 4 inches diameter, were bored near the centre of the fore-head. These were not charged and exploded, their purpose being to weaken the surrounding rock. The remainder were charged, those adjoining the centre being first fired, and the result of these explosions was a cavity. The remaining holes were then exploded from this cavity outwards.

The workmen were industrious under circumstances which required more than ordinary perseverance. A premium on their wages was given for more than a certain advancement per day. At the time of the author's visit, one metre per day was the standard. For a progress of

$1\frac{1}{10}$ metre per day	. .	$1\frac{1}{10}$ day's wages was paid.
$1\frac{1}{5}$ " " "	. .	$1\frac{1}{5}$ " " "
$1\frac{3}{10}$ " " "	. .	$1\frac{3}{10}$ " " "
$1\frac{4}{10}$ " " "	. .	$1\frac{1}{2}$ " " "

This scale was subject to adjustment every fortnight.

The déblais resulting from the explosion of the eighty holes was removed in small wagons. Its removal was well organized, and considering the circumstances, quickly effected. It would be much accelerated, if it was possible to construct an iron frame, strong enough to be placed close to the fore-head at the time of the explosion, and receive without injury the products of explosion, which could be removed *en masse*, or nearly so.

Two descriptions of machines for compressing air were in use,—one on the hydraulic ram principle, the other resembling a pump. In the first, the water was admitted, with a pressure of $85\frac{1}{4}$ feet, into a column or vessel, containing air, about 14 feet high and 2 feet in diameter. The water by its momentum rushed up the column, compressed the volume of air, and forced it through a valve into a reservoir. The pressure valve being closed, the exhaust valve was opened and the water fell in the column at the same time, its place was taken by air, and the machine became ready for another stroke. This machine made $2\frac{1}{2}$ strokes per minute, and was capable of supplying about 20 cubic feet of air, compressed to five atmospheres per minute. The other machine consisted of a horizontal pump and two vertical branches. The piston was surrounded by water, which rose and fell alternately in two columns: when it rose, compressing the air, and forcing it through the outlet valve; and when it fell, creating a vacuum, which was filled by air at atmospheric pressure.

The tunnel, on the 30th June, 1863, had been driven (including the advanced gallery) at Modane 1092·25 metres, and at Bardonnèche 1450·00 metres. The advancement in June last, at Modane, was at the rate of 4·719 feet per day. At this rate of progress at both ends, the tunnel would be finished in 9 years 2½ months from that time. It was not, however, too much to expect a progress of two metres per day at each end, seeing that machines had only been in use at Bardonnèche about two years and a half, and at Modane half a year. A great part of that time had been taken up in experiments, and the men were not thoroughly habituated, as yet, to the manipulation of the machines. The machines were also being much improved. With an average rate of 2 metres per day, from June 30th, 1863, 6 years and 7 months would be required for the completion of the tunnel, as compared with 26 years 3 months by hand-labor, at 1·655 feet per day at each end, the average rate of progress previous to the introduction of the machinery. The machinery in use at Mont Cenis was made for the greater part at Seraing, near Liège. M. Sommeiller confidently expected, an advancement of three metres per day at each end. If that were the average from June 30th, 1863, the work would be completed in 4 years 8½ months from that time.

In the advanced gallery at Modane, the number of workmen employed during the twenty-four hours was a follows:—

88 machinists, in two sets of 44 each.
9 chargers in one set.
30 laborers for removing the dèblais, in one set.

127
344 men were also engaged in enlarging and walling, giving a total employed underground of—

471, and including blacksmiths, stone-dressers, and other laborers at the surface, there were employed at the tunnel, . . . 700
Mechanics, brakesmen, &c., in the workshops, machinery, &c. . 240
Occasional laborers, ... 200

Or a total at Modane of 1140

At Bardonnèche the number was greater; 1200 to 1400 being generally employed, giving a total of 2540 on the works.

The result of a rough comparison was to show that in the present development of the Sommeiller system, an advancement three times quicker than by hand-labor might be effected, but at about two and a half times the cost; judging rather of places where it might be generally applied than by Mont Cenis only. The proportion of two and a half to one increase of cost, referred only to what was known as mining charges in the advanced gallery, *i. e.*, wages, tools, candles, and gunpowder. This proportion was notably diminished in the case of a railway tunnel, where enlarging, timbering, walling, laying of rails, &c., were charges common to both systems. In the case of a tunnel, through rock, costing when completed £30 per yard, the two systems might compare as follows :—an increased advancement in favor of machinery of 3 to 1, at an increased cost of 4 to 3.

The ventilation was good in the advanced gallery, the exhaust air from the machines afforded an ample supply. During the time of exploding the holes, a jet of air was left open. Further back, where the men were employed in enlarging the tunnel, the ventilation was insufficient. The tunnel was therefore being divided with a horizontal brattice,—the upper section being in communication with the chimney on the mountain side. The air was intended to pass along the under-side of this division, and then return by the upper part, which had an area of 7 square metres.

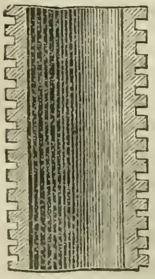
The works were now performed at the charge of the Italian Government. On their completion the French Government was to pay £760, 000 for that portion of the tunnel situated in its territory—one-half the entire length—together with a premium of £20,000 for each year by which a term of 25 years counting from January 1st, 1862 was reduced. This premium would be increased to £24,000 for each year by which a term of 15 years was reduced, counting from the same time. In addition, the French Government would pay interest at five per cent. per annum on such portion of the tunnel as was finished. If, however, the Italian Government did not complete the work within twenty-five years from the time of making the agreement, or if they renounced the works before that time, the French Government was absolved from further payment. If the works were finished, as there was every reason to suppose they would be, in ten years from June 30th, 1863, the French Government would pay £1,287,000 for the construction of one-half of the tunnel, or at the rate of £210 per metre.

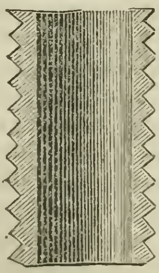
Proc. Inst. Civil Eng., Feb. 16, 1864.

Locomotive Improvement.

From the London Mechanics' Magazine, Sept., 1863.

SIR :—Much has already been done to increase the heating surface of steam boilers, but I think a further improvement may be effected by increasing the *surface in contact with the water,* without a further increase of the absorbent surface.





It is pretty generally known that flanges cast on air warming apparatus, as applied by the London Warming and Ventilating Company, greatly increase the effect of such apparatus, the cause being that a greater surface of heated metal is in contact with the air. I think, therefore, that if the metallic surface of tubes, flues, &c., of boilers, in contact with the water, be increased in area, an increase of evaporation will ensue. This may be effected in several ways, varying with the form of boiler; but, in the case of locomotive boilers, the two modes shown in the accompanying sectional diagrams, might be found most simple and effective.

Neither the thickness or the weight of the tubes need be much, if at all, increased. T. MOY.

CLIFFORD'S-INN, Sept., 5, 1863

Experiments to determine the effects of Impact Vibratory Action, and a Long-continued change of load on Wrought Iron Girders. By WILLIAM FAIRBAIRN, LL.D., F.R.S.

From the London Artizan, April, 1864.

The following is an abstract of Mr. Fairbairn's communication. The author observed that the experiments which were undertaken, nearly twenty years ago, to determine the strength and form of the Tubular Bridges which now span the Conway and Menai Straits, led to the adoption of certain forms of girder, such as the tubular, the plate, and the lattice-girder, and other forms founded on the principle developed in the construction of these bridges. It was at first designed that the ultimate strength of these structures should be six times the heaviest load that could ever be laid upon them, after deducting half the weight of the tubes. This was considered a fair margin of strength; but subsequent considerations, such as generally attend a new principle of construction with an untried material, showed the expediency of increasing it; and instead of the ultimate strength being six times, it was in some instances increased to eight times the weight of the greatest load.

The proved stability of these bridges gave increased confidence to the engineer and the public, and for several years the resistance of six times the heaviest load was considered an amply sufficient provision of strength.

But a general demand soon arose for wrought iron bridges, and many were made without due regard to first principles, or to the law of proportion necessary to be observed in the sectional areas of the top and bottom flanges, so clearly and satisfactorily shown in the early experiments. The result of this was the construction of weak bridges, many of them so ill-proportioned in the distribution of the material as to be almost at the point of rupture with little more than double the permanent load. The evil was enhanced by the erroneous system of contractors tendering by weight, which led to the introduction of bad iron, and in many cases equally bad workmanship.

The deficiencies and break-downs which in this way followed the first successful application of wrought iron to the building of bridges, led to doubts and fears as to their security. Ultimately it was decided by the Board of Trade that in wrought iron bridges the strain with the heaviest load should not exceed 5 tons per square inch; but on what principle this standard was established does not appear.

The requirement of 5 tons per square inch did not appear sufficiently definite to secure in all cases the best form of construction. It is well-known that the power of resistance to strain in wrought iron are widely different, according as we apply a force of tension or compression; it is even possible so to disproportion the top and bottom areas of a wrought iron girder calculated to support six times the rolling load, as to cause it to yield with little more than half the ultimate strain or 10 tons on the square inch. For example, in wrought iron girders with solid tops, it requires the sectional area in the top to be nearly double that of the bottom to equalize the two forces of tension and compression ; and unless these proportions are strictly adhered to in the construction, the 5-ton strain per square inch is a fallacy which may lead to dangerous errors. Again, it was ascertained from direct experiment that double the quantity of material in the top of a wrought iron girder was not the most effective form for resisting compression. On the contrary, it was found that little more than half the sectional area of the top, when converted into rectangular cells, was equivalent in its powers of resistance to double the area when formed of a solid top plate. This discovery was of great value in the construction of tubes and girders of wide span, as the weight of the structure itself (which increases as the cubes, and the strength only as the squares) forms an important part of the load to which it is subjected. On this question it is evident that the requirements of a strain not exceeding 5 tons per square inch cannot be applied in both cases, and the rule is therefore ambiguous as regards its application to different forms of structure. In that rule, moreover, there is nothing said about the dead weight of the bridge ; and we are not informed whether the breaking weight is to be so many times the applied weight plus the multiple of the load, or, in other words, whether it includes or is exclusive of the weight of the bridge itself.

These data are wanting in the railway instructions ; and until some fixed principle of construction is determined upon, accompanied by a standard measure of strength, it is in vain to look for any satisfactory results in the erection of road and railway bridges composed entirely of wrought iron.

The author was led to inquire into this subject with more than ordinary care, not only on account of the imperfect state of our knowledge, but from the want of definite instructions. In the following experimental researches he has endeavored to ascertain the extent to which a bridge or girder of wrought iron may be strained without injury to its ultimate powers of resistance, or the exact amount of load to which a bridge may be subjected without endangering its safety—in other words, to determine the fractional strain of its estimated powers of resistance.

To arrive at correct results and to imitate as nearly as possible the strain to which bridges are subjected by the passage of heavy trains, the apparatus specially prepared for the experiments was designed to lower the load quickly upon the beam in the first instance, and next to produce a considerable amount of vibration, as the large lever with its load and shackle was left suspended upon it, and the apparatus was sufficiently elastic for that purpose.

The girder subjected to vibration in these experiments was a wrought iron plate beam of 20 ft. clear span, and the following dimensions:—

Area of top, . . .	4·30 square inches.
Area of bottom, . .	2·40 "
Area of vertical web, . .	1·90 "
Total sectional area, . .	8·60 "
Depth, . . .	16 inches.
Weight, . . .	7 cwt. 3 qrs.
Breaking-weight (calculated), .	12 tons.

The beam having been loaded with 6643 lbs., equivalent to one-fourth of the ultimate breaking-weight, the experiments commenced as follows:—

EXPERIMENT I.—*Experiment on a wrought iron beam with a changing load equivalent to one-fourth of the breaking-weight.*

Date.	Number of changes of Load.	Deflection produced by Load.	Remarks.
1860.			
March 21,	0	0·17	Strap loose on the 24th March.
April 7,	202,890	0·17	Strap broken on the 20th April.
May 1,	449,280	0·16	
May 14,	596,790	0·16	

The beam having undergone about half a million changes of load by working continuously for two months night and day, at the rate of about eight changes per minute, without producing any visible alteration, the load was increased from one-fourth to two-sevenths of the statical breaking-weight, and the experiments were proceeded with till the number of changes of load reached a million.

EXPERIMENT II.—*Experiment on the same beam with a load equivalent to two-sevenths of the breaking-weight, or nearly 3½ tons.*

Date.	Number of changes of Load.	Deflection in inches.	Remarks.
1860.			
May 14,	0	0·22	In this experiment the number of changes of load is counted from 0, although the beam had already undergone 596,790 changes, as shown in the preceding Table.
May 22,	85,820	0·22	
June 9,	236,460	0.21	
June 26,	403,210	0·23	The beam had now suffered one million changes of load.

After the beam had thus sustained one million changes of load without apparent alteration, the load was increased to 10,486 lbs., or $\frac{2}{5}$ths of the breaking-weight, and the machinery again put in motion. With this additional weight the deflections were increased, with a permanent set of ·05 in., from ·23 to ·35 in., and after sustaining 5175 changes, the beam broke by tension at a short distance from the middle. It is satisfactory here to observe that during the whole of the 1,005,175 changes none of the rivets were loosened or broken.

The beam broken in the preceding experiment was repaired by replacing the broken angle-irons on each side, and putting a patch over the broken plate equal in area to the plate itself. A weight of 3 tons was placed on the beam thus repaired, equivalent to one-fourth of the breaking-weight, and the experiments were continued as before.

EXPERIMENT III.

Date.	Number of changes of Load.	Deflection in inches.	Permanent set in inches.	Remarks.
1860. August 9,	158	. .	. .	The load during these changes was equivalent to 10,500 lbs., or 4·6875 tons at the centre. With this weight the beam took a large but unmeasured set.
August 11,	12,950			During these changes the load in the beam was 8025 lbs., or 3·58 tons.
August 13,	24,900	0·22	?	
August 13,	25,900	0·18	0	
December 1,	768,100	0·18	0·01	Load reduced to 2·96 tons, or ¼th the breaking-weight.
1861. March 2,	1,602,000	0·18	0·01	
May 4,	2,110,000	0·17	0·01	
September 4,	2,727,754	0·17	0·01	
October 16,	3,150,000	0·17	0·01	

At this point, the beam having sustained upwards of 3,000,000 changes of load without any increase of the permanent set, it was assumed that it might have continued to bear alternate changes to any extent with the same tenacity of resistance as exhibited in the foregoing Table. It was then determined to increase the load from one-fourth to one-third of the breaking-weight; and accordingly 4 tons were laid on, which increased the deflection to ·20

EXPERIMENT IV.

Date.	Number of changes of Load.	Deflection in inches.	Permanent set in inches.	Remarks.
1861. October 18,	0	0·20	0	
November 18,	126,000	0·20		
December 18,	237,000	0·20		
1862. January 9,	313,000	. .	. .	Broke by tension across the bottom web.

Collecting the foregoing series of experiments, we obtain the following summary of results:—

SUMMARY OF RESULTS.

No. of Expt.	Date.	Weight on middle of the beam in tons.	Number of changes of Load.	Strain per square inch on bottom.	Strain per square inch on top.	Deflection, in inches.	Remarks.
1	From March 21 to May 14, 1860,	2·	596,790	4·62	2·58	·17	
2	From May 14 to June 26, 1860,	3·50	403,210	5·46	3·05	·23	
3	From July 25 to July 28, 1860,	4·68	5,175	7·31	4·08	·35	Broke by tension a short distance from the centre of the beam.

BEAM REPAIRED.

4	Aug. 9, 1860,	4·68	158	7·31	4·08	. .	The apparatus was accidentally set in motion.
5	Aug.11 & 12,	3·58	25,742	3·59	3·12	·22	
6	From Aug. 13, 1860, to Oct 16, 1861,	2·96	3,124,100	4·62	2·58	·18	
7	From Oct. 18, 1861, to Jan. 9, 1862,	4·00	313,000	6·25	3·48	·20	Broke by tension as before, close to the plate riveted over the previous fracture.

From these experiments it is evident that wrought iron girders of ordinary construction are not safe when submitted to violent disturbances equivalent to one-third the weight that would break them. They, however, exhibit wonderful tenacity when subjected to the same treatment with one-fourth the load; and assuming therefore that an iron girder bridge will bear with this load 12,000,000 changes without injury, it is clear that it would require 328 years at the rate of 100 changes per day before its security was affected. It would, however, be dangerous to risk a load of one-third the breaking-weight upon bridges of this description, as, according to the last experiment, the beam broke with 313,000 changes; or a period of eight years, at the same rate as before, would be sufficient to break it. It is more than probable that the beam had been injured by the previous 3,000,000 changes to which it had been subjected; and assuming this to be true, it would follow that the beam was undergoing a gradual deterioration which must sometime, however remote, have terminated in fracture.

A Bridge over the Straits of Messina.

From the Lond. Practical Mechanic's Journal, May, 1864.

According to the Italian journals, it is in contemplation to unite Sicily to the main land by a bridge. The bridge proposed would be a suspension one, on a new system, the chains being of cast steel, and strong enough to support the weight of several railway trains.

MECHANICS, PHYSICS, AND CHEMISTRY.

Compressed Asphalte.

From the Lond. Mechanics' Magazine, April, 1864.

At a recent meeting, M. Malo read a very interesting paper, "On Compressed Asphalte as a material for roads," before the Society of Civil Engineers, at Paris. After referring to the defects inherent in all the old systems of paving adopted in the French capital, M. Malo treated of the attempts which had from time to time been made to substitute something better. Of all the systems tried, however, he stated that but one had borne the requisite tests, and that after a trying novitiate, extending over a period of ten years, compressed asphalte now took a place as a powerful rival to ordinary stone paving and Macadam.

The asphalte employed for the works already executed in Paris is a pure carbonate of lime, naturally impregnated with from six to ten per cent. of bitumen. The rock is quarried in regular beds, four to seven yards thick, at Seyssel (Ain), Val de Travers (canton de Neufchatel), and in several other places in the Jura. At a temperature equivalent to that of boiling water, the bitumen softens so much that the stone crumbles to powder; if now this powder, while still hot, be powerfully compressed, it will form masses possessing, when cold, an amount of hardness equal to that of the unquarried rock, and it is this peculiar property which has been somewhat recently applied on an extended scale, to the formation of roadways in Paris.

M. Malo stated that the crude asphalte is first broken by mechanical means into small pieces, then reduced to powder, and subsequently placed in large iron cauldrons, wherein it is heated to about 140 degrees Cent. While thus hot it is carried quickly in suitable ladles, to the locality where it is to be employed. The proper curved form which the finished road is intended to assume has been previously imparted to a bed of concrete (*Beton*), on which the hot asphalte, in powder, be it observed—for the pressure is needed to make it agglomerate—is spread, and carefully rammed, with heated cast iron rammers, into a solid sheet, so to speak. Three heavy rollers are then passed successively over the gradually hardening roadway. The first weighs about 5 cwt., the second 1 ton, and the third roller about 2 tons 5 cwt. By this means the stratum of asphalte is reduced to a uniform thickness, fixed in Paris at four centimetres. Two or three hours after the passage of the last roller the material has become so far cooled and consolidated that traffic can be freely resumed on its surface.

In 1850, one year after the discovery of this process, M. Darcy, inspector general of roads, proposed its application to a portion of the boulevards; but it was not until 1854 that the first piece of compressed asphalte pavement was put down, in the Rue Bergère, under the superintendence of MM. Homberg, chief engineer, and Vaudry, engineer in ordinary in the municipal service. In 1854 we find that about 700 or 800 metres only of the new roadway were in existence. In 1858 the area had increased, to 8000 metres, and now it is more than 100,-

000 metres, without including many large court-yards, for which the new pavement has been selected, less for the sake of solidity than for the absence of noise which follows on its use. As in all other new things, M. Malo stated, that many mistakes were made at first, many mishaps met with, and dfficulties overcome. We will mention a few of the more important.

The first essential was obviously to discover a good method of preparing the material. After months of labor and care this object seemed as far from being obtained as ever. Then followed the difficulties of application, and in this department the experiments conducted in the Rue Neuve Petits Champs were fertile in instruction. The asphalte used was hard and unmanageable ; the season far advanced, and it was a matter of some difficulty to procure a dry surface of concrete for the asphalte to rest on. The moisture was evaporated by the heat, and, pervading the mass, prevented the efficient agglomeration of its particles. Worst of all, the concrete itself reposed on soil recently disturbed in the formation of a sewer, and the settlement which followed, as a matter of course, led to the rupture of the bed of asphalte. Ultimately all these obstacles were overcome, and M. Malo states the advantages of the new roadway as follows:—It produces neither mud nor dust ; the annual wear equalling only one millimetre, once it has become thoroughly consolidated by the passage of vehicles. It is almost perfectly noiseless--no trifling advantage, be it remarked. What would not we dwellers in London give for a noiseless pavement ? The labor of horses is materially lessened on the compressed asphalte, as compared with stone paving or Macadam ; a fact disputed, however, by M. Tresca, a gentleman who has conducted a valuable series of experiments on the tractive resistances of various roads. The expense of maintaining wheel carriages in repair is considerably reduced by the suppression of ruts and jolting. M. Malo stated the saving at 8,500,000 francs, supposing all Paris asphalted, yearly; and, lastly, the absence of vibration tends to the durability of the houses on either side. In opposition, it has been urged that the new pavement is too slippery for smoothly shod carriage and saddle horses (*chevaux de Luxe*). According to M. Malo, this inconvenience is never experienced unless the curvature of the roadway is exaggerated, or on steep hills. This has been verified by experience. It was found that one horse in 1308 fell in passing through the Rue de Sèze, which is paved, and but one in 1409 in passing through the Rue Neuve des Capucin, which is asphalted. Sometimes, however, the surface is rendered slippery by the presence of foreign substances dropped in traffic. A simple washing removes the evil. The same end may be attained by means of a slight sprinkling of sand.

M. Malo estimated the cost of the different roadways used in Paris, at the following rates per square metre :—

Material.	Formation.	Annual Maintenance.
Compressed Asphalte and Concrete,	15 f.	1 f. 25 c.
Pavement of Belgian Porphyry,	10 f. to 22 f.	0 f. 50 c. to 1 f. 50 c.
Macadam,	7 f.	2 f. 40 c. to 3 f.

M. Malo concluded by stating that all the Jura district, from the department of the Bas Rhine to Savoy, abounds in asphalte, so that no fears need be entertained of exhausting the supply by extending the application of the material.

Cement for Steam Joints, &c.

From the London Builder, No. 1073.

Plumbago has recently been introduced as the basis of a cement for steam-joints, and the general metallic connexions of the engineer. It is composed of six parts of plumbago, three of slacked lime, eight of sulphate of baryta, and three of boiled linseed oil. This compound, it is said, secures a perfectly air and steam-tight joint, much superior to that obtained by the use of red lead.

Resistances to Bodies passing through Water. By Mr. G. H. Phipps.

[Read before the Insti. of Civil Eng., March 8 and 15, 1864.]

From the London Mechanics' Magazine, March, 1864.

These resistances comprised the plus resistance, or that concerned in moving out of the way the fluid in advance of the body; the minus resistance, or the diminution of the statical pressure behind any body when put into a state of motion in a fluid; and the frictional resistance of the surface of the body in contact with the water.

The plus resistance of a plane surface one foot area, moving at right angles to itself in sea water, was considered to be

$\text{R} = \frac{64{\cdot}2 \times v_2}{2\,g}$, and the minus resistance was one-half the plus resistance.

For planes moving in directions not at right angles to themselves, theoretical resistances were, for the plus pressure—

$$\text{S} = \frac{a}{r^2}, \text{ and } \text{R} = \frac{\text{S}\,64{\cdot}2\,v^2}{2\,g}$$

the minus pressure being one-half the above; where R was the resistance of the inclined plane; a, the area of the projection of the inclined plane upon a plane at right angles to the direction of motion; r, the ratio of the areas of the projected and the inclined planes; and S, the area of a square-acting plane of equivalent resistance with the inclined plane.

But, besides these theoretical resistances, the experiments of Beaufoy showed, that when the inclined planes were of moderate length only, the plus resistance was considerably in excess of the above; so that when the slant lengths of the planes were to their bases in the proportion of 2 to 1, 3 to 1, 4 to 1, and 6 to 1,

the actual resistances exceeded the theoretical, as

1·1 to 1, 1·98 to 1, 3·24 to 1, and 6·95 to 1.

The author proposed a method of approximating to these additional resistances, by adding the constant fraction $\frac{1}{7}$th of a square foot for every foot in depth of the plane, to the quantity S previously determined, which empirical method he found to agree nearly with the results of Beaufoy's experiments.

The resistances of curved surfaces, such as the bows of ships, were adverted to, the method of treating them being to divide the depth of immersion into several horizontal layers, and then again into a number of straight portions, and to deal with each portion as a separate detached plane, according to the preceding rules.

The question of friction was then considered. The experiments of Beaufoy were referred to, giving 0·339 ℔s. per square foot as the co-efficient of friction for a planed and painted surface of fir, moved through the water at 10 feet per second, the law of increase being nearly as the squares of the velocities, viz: the 1·949 power. The author was, however, of opinion that a surer practical guide for determining the co-efficient of friction would be, by considering all the data and circumstances of a steamship of modern construction, moving through the water at any given speed. The actual indicated horse-power of the engines being given, the slip of the paddles being known, and the friction and other losses of power approximated to, it was clear that the portion of the power necessary to overcome the resistance of the vessel might be easily deduced. By determining approximately, by the preceding rules, the amounts of the plus, the minus, and the additional head resistances, and deducting them from the total resistance, the remainder would be the resistance due to the friction of the surface. By this process, and taking as an example, the iron steamship "Leinster," when perfectly clean, and going on her trial trip 30 feet per second in sea water, her immersed surface being 13,000 sq. ft. the co-efficient of friction came out at 4·34 ℔s. per sq. ft.* Beaufoy's co-efficient of 0·339 ℔. per square foot at 10 feet per second, would, according to the square of the velocities amount to 3·051 ℔s. at 30 ft. per second. The difference between this amount and the above 4·34 ℔s. might be accounted for by a difference in the degree of roughness of the surfaces.

Other methods for the determination of the co-efficient of friction were then discussed. One, derived from the known friction of water running along pipes, or water courses, was shown to be considerably in excess of the truth. It was founded upon the observed fact, that, at a velocity of 15 feet per second, the friction of fresh water on the interior of a pipe was 25 ozs.† per square foot. Applying this to the ship "Leinster," and increasing the friction as the squares of the velocities up to 30 feet per second, the above friction would become 100 ozs., or $6\frac{1}{4}$ ℔s. per square foot, which acting upon, 13,000 square feet of surface, would absorb, at the above speed no less than 4396 H.P., whilst the total available power of the engines, (after deducting from the indicated 4751 H.P. $\frac{1}{10}$th for friction, working air-pumps, and other losses, and ⅕th of the remainder for the observed slip,) was only 3421 H.P.; that showing an excess of resistance equal to 974, H.P., without allowing any power to overcome the other resistances. The assumption of 25 ozs. being the proper measure of the friction per sq. ft. at a velocity of 15 ft. per second, upon the clean surface of an iron ship, seemed to have arisen from the opinion, very gererally entertained, that there was no difference in the amount of friction in pipes and water-courses, whether internally smooth like glass, or moderately rough like cast iron, and that the surfaces of ships were subject to the same action. The comparatively recent experiments, in France, of the late M. Henry Darcy, were in opposition to the above view, and showed that the condition as to roughness of the interior of a pipe modified the friction considerably. Thus, with three different conditions of surface, the co-efficients were:—

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* Since the paper was written, the accuracy of the above co-efficient had been confirmed very nearly, by an examination of another vessel, the "Atrata" of different type from the "Leinster" the calculations of which were given in an Appendix.

† For sea-water this quantity must be increased as the specific gravity, or as 62·5 to 64·2.

A. Iron plate covered with bitumen made very smooth,	0·000432
B. New cast iron,	0·000584
C. Cast iron covered with deposits,	0·001167

The friction was, therefore, nearly as 1, 1½ and 3.

As there appeared no reason to doubt the correctness of M. Darcy's experiments, even in pipes the notion of the friction being uninfluenced by the state of roughness of the interior could no longer be entertained. The 25 ounces, previously mentioned as the measure of friction per square foot for the interior of pipes and water-courses, could not, therefore, be regarded as a constant quantity, applicable to all kinds of surfaces; but from the author's calculations, it appeared to come in intermediately between the co-efficients of the surfaces B and C, given in the above scale; as at 15 feet per second.

A	would give	13⅓	ounces per	square foot.
B	"	20	"	"
C	"	40	"	"

Besides, there was another cause for an excess of friction in pipes and water-courses, over that upon ships, even when the surfaces were equally smooth. It arose from the circumstance, that where the velocity of the water in a pipe, or open water-course, was spoken of, the meaning was, its average velocity; whilst the velocity of a vessel through still water meant what the words implied, namely, the relation of the vessel's motion to the fluid at rest. If the case were taken of a water-course of such width, that the friction of the bottom only need be considered, with an average velocity of flow of 15 feet per second, the friction upon the bottom would be equal to 25 ozs. per square foot.; but according to the rules generally used, an average velocity of 15 ft. per second corresponded to a surface velocity of 16·66 feet per second which was the velocity with which a vessel should pass through still water, to give an equal friction upon its sides. According to Beaufoy, the velocity of 16·66 feet per second would produce a friction of ·932 lbs. or 14·91 ozs., where 15 feet would only give 12·2 ozs. The difference between the 14·91 ozs. and 25 ozs. (equal to 10·09 ozs.) must therefore, be set down to the different degree of roughness of the surfaces in the water-course and the vessel.

Taking, then, 4·34 lbs. as the friction per square foot of a new iron ship, moving through the water at a speed of 30 feet per second, it would be found that this was equal to the 1-207·06th part of the plus resistance of a plane 1 foot square, moving through the water at right angles to itself at the above velocity. Also, as the resistance of both planes increased according to the same law of the square of the velocities, the ratio of 1 to 207·06 would subsist at all velocities.

$$\text{The ratio was as } \frac{64\cdot2\,v_2{}^2}{2g} \text{ to } 4\cdot34 \text{ lbs.} = \frac{1}{207\cdot06}$$

Calling the ratio r, and the whole fractional surface in square feet s, and S, as before, the area of a square-acting plane of equivalent resistance, then $S = s \div r = s \div 207\cdot06$.

As an example of the application of the previous deductions, the performance of the steamship "Leinster," on her trial trip, when going through sea-water at a speed of 30 feet per second was referred to. In this case—

m, the area of the immersed midship section was .	336 sq. ft.
d, the draft of water, . . .	13 ft.
r, the reduced ratio of the slant length of the bow to the projection, . . .	10 to 1.
r', the same for the stern, . . .	10 to 1.
r', the ratio of one square foot of square-acting plane, to 1 square foot of frictional surface,	207·06 to 1.
v, the velocity in feet per second, . .	30
w, the weight of a cubic foot of sea water, .	64·2 lbs.
f, the area of the frictional surface, .	13,000 sq. ft.

Calling P, the plus, or head resistance; M, the minus, or stern resistance; A, the additional head resistance; F, the fractional, or surface resistance; S, the area of a square-acting plane having an equal resistance with each of the above; and R, the total resistance;

$$\text{Then, } P = \frac{m}{r^2} = S = \frac{336}{100} = 3\cdot36 \text{ sq. ft.}$$

$$\text{"} \quad M = \tfrac{1}{2}\frac{m}{r^2} = S = \tfrac{1}{2}\frac{336}{100} = 1\cdot68 \quad \text{"}$$

$$\text{"} \quad A = \frac{13}{7} = S = \quad 1\cdot86 \quad \text{"}$$

$$\text{"} \quad F = \frac{13000}{207\cdot06} = S = \quad 62\cdot78 \quad \text{"}$$

$$S = \quad \overline{69\cdot68} \quad \text{"}$$

$$R = 69\cdot68 \times \frac{64\cdot2\,v^2}{2\,g} = 69\cdot68 \times 900 = 62{,}712 \text{ lbs.}$$

$$H \text{ (Realized power)} = 62{,}712 \text{ lbs.} \times 30 \div 550 = 3420\cdot66 \text{ H.P.}$$

$$H' \text{ (Gross power) including the slip and other losses, } = 3420\cdot66 \times \frac{100}{72}$$

$$= 4751 \text{ H.P.}$$

Thus, by ascertaining the value of S for any vessel, which was entirely independent of velocity, it would be easy to determine the power necessary to propel it at any required speed, or the speed being given, to find the corresponding power.

$$\text{Generally } H = VS\frac{64{\cdot}2\ V^2}{2\ g} \div 550. \qquad . \qquad . \qquad (1)$$

Or, because for sea-water 64·2 was very nearly equal to 2 g,

$$H = \frac{V^3\ S}{550} \qquad . \qquad . \qquad . \qquad (2)$$

When the slip and other losses were in the same proportion as in the "Leinster."

$$^{*}H' = H\frac{100}{72} \qquad . \qquad . \qquad (3)$$

When the gross power was given, and the velocity was required:—

$$^{\dagger}V = \left\{\frac{\frac{72}{100}\ H' \times 550}{S\ \frac{64{\cdot}2}{2g}}\right\}^{\frac{1}{3}} \qquad . \qquad (4)$$

The author then proceeded to examine the question of the influence of form in reducing the resistance of vessels.

It was argued that, in vessels of similar type to the "Leinster," where $\frac{9}{10}$ths of the whole resistance was due to friction, and only $\frac{1}{10}$th to considerations involving the question of "form," no minor modifications of the latter could have much effect in diminishing the total resistance. The case of other vessels of different type, more bluff in the bows and not so fine in the run, was adverted to, and a particular instance was discussed, where the inertial resistance was supposed to be equal to one-fifth of the total resistance, and the slant length of the bows to the base to be as 6 to 1. If such a vessel were altered, so as to make the above proportion $8\frac{1}{2}$ to 1, the improvement would only diminish the total resistance by $\frac{1}{10}$th.

In opposition, however, to this view, the author referred to several ships, the particulars of which were given in the discussion upon Mr. Armstrong's Paper on "High Speed Steam Navigation," (Minutes of Proceedings, Inst. C. E. vol. xvi.) These vessels were the "Rifleman," the "Teazer," the "Dwarf," the "Magnet," the "Flying Fish," and two vessels by Mr. Scott Russell. By moderate improvements in the bows and run of the above vessels, the resistance was diminished to degrees varying from $\frac{1}{2}$ to $\frac{1}{9}$th of the whole, a considerably greater improvement than could be accounted for upon the principles of calculation in the Paper.

Without attempting to solve the difficulty, the author threw out, as a suggestion for examination, whether, as nearly all the above vessels were propelled by screws, the improvements noted might not be referable to an improved action in the propelling instrument, brought about by an alteration in the hulls of the vessels, and the question suggested itself whether, if the resistances of the vessels in question could have been ascertained by means of dynamometer applied to a towing-rope, the improvement would have been so great, as when shown by the diminished power of the engines.

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[*] If for fresh water $H' \times 0{\cdot}97 =$ gross H.P.

[†] If for fresh water $V = 0{\cdot}99 =$ velocity.

For the Journal of the Franklin Institute.

Economy in the Use of Steam. By D. M. Greene, C. E. Second Assist. Eng. U.S.N.

The special attention which has been recently attracted to this important subject, and the interest in it, which has been developed in popular no less than in professional minds, together with the wholly unsatisfactory and unprofitable popular discussions in relation to it, have induced the writer to undertake a reconciliation of the results of *experiment* with those of *theory*.

The experimental results alluded to, are the conclusions arrived at in the case of the experiments made some years since, at Erie, Pa., under the auspices of the United States Government, for the purpose of ascertaining the relative costs of net power, when steam is used with different measures of expansion; conclusions which are generally assumed to be entirely at variance with the theory hitherto almost universally accepted as true.

In the discussion which is to follow, the law of Mariotte is accepted as practically governing the case; except so far as it is necessarily modified by the comparatively recent discovery, that when heat is employed as a motive force, or "mode of motion," the work done is due to the transmutation of a definite amount of heat into work. According to the theory based upon the discovery alluded to, and which is now generally accepted by competent authorities everywhere, as correct, whenever an amount of work, equivalent to 772 pounds raised 1 foot, is accomplished through the agency of heat, an amount of heat sufficient to raise 1 pound of water 1° Fahr. is transmuted into that work; in other words, the mechanical equivalent of a unit of heat* is 772 pounds raised 1 foot high; or, it is equal to a force of 772 pounds exerted over a space of 1 linear foot.

Hence for each horse power developed by a steam engine, there are $\frac{33000}{772} = 42{\cdot}746$ units of heat transmuted into power; and as a necessary consequence, such transmutation must be attended by a certain, definite, *condensation* in the cylinder; whence we are forced to the conclusion, that the volume of steam which enters the cylinder during each stroke must be greater than the space displaced by the piston, by the volume condensed to produce the power developed.

As a simple example, showing the extent of condensation due to the production of power in a particular case, take a cylinder whose piston has an area of 3300 square inches, 10 feet stroke, and makes 25 single strokes per minute; using a steam pressure of two atmospheres, or 29·4 pounds above zero. Then the total power developed, will be $\frac{3300 \times 29{\cdot}4 \times 10 \times 25}{33000} = 735$ horses; and the heat transmuted into power, $735 \times 42{\cdot}746 = 31418$ units.

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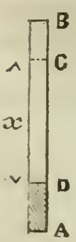
* By "unit of heat," is of course meant an amount of heat sufficient to raise 1 lb. of water 1° Fahr.

Now the total heat of steam of two atmospheres is 1190°; and its sensible temperature 250°: hence $\frac{31418}{1190 - 250} = 33{\cdot}4$ pounds, or $\frac{33{\cdot}4 \times 841}{62{\cdot}5} = 449$ cubic feet of steam will be condensed per minute. During the same time, the space displaced by the piston will be 5729 cubic feet, and the total volume of steam used, $5729 + 449 = 6178$ cubic feet; of which $\frac{449 \times 100}{6178} = 7{\cdot}26$ per cent. is condensed to produce the power.

In the preceding example, we have supposed the steam to follow full stroke; when high measures of expansion are employed, the condensation—being proportional to the work done—will, of course, be largely increased; as, for example, when steam is expanded four times, the condensation will be $2{\cdot}38\left(\frac{449 \times 100}{5729}\right) = 18{\cdot}66$ per cent. of the space displacement of the piston; so that, in order to realize the 2·38 units of work usually shown by theory to have been performed, the volume of steam admitted to the cylinder to do the work, and to preserve the tension must be $\frac{100 \times 100}{81{\cdot}34} = 1{\cdot}23$ units, instead of a single unit. It follows, therefore, that the gain in work, instead of being 138 per cent. of that realized when steam is used without expansion, is only $\frac{(238 - 123)\,100}{123}$ 93·5 per cent. We might easily modify this gain by the loss due to clearance, and by the constant prejudicial resistances of back pressure, and the friction of the engine itself; but we prefer to consider the general case, and deduce a formula which will give the *cost of a net horse-power, in pounds of water evaporated per hour*, under any and every variety of practical conditions, as regards pressure, expansion, clearance, back and friction pressures; enough however, has been said, to awaken a suspicion in the intelligent mind, that the extravagant gains promised by theory—as applied and heretofore generally relied upon—are but myths.

In order to best accomplish our purpose, it is necessary, in the first place, to calculate the

Mechanical Effect available from the Evaporation of 1 *pound of water per hour, the steam being used without Expansion.*



Let AB, Fig. 1, be a tube of indefinite length, having a sectional area equal to 1 square inch: suppose it to contain a column of water one foot high: upon this water let a piston rest without weight, and capable of motion without friction. Then, when this filament of water has been converted into steam, under the pressure of the atmosphere, the piston will have been raised through a height $DC = x$; and an amount of work $W = 14{\cdot}7\,x,$

will have been done: in which x will represent the specific volume of steam of one atmosphere, less the condensation due to the production of the work $+1$. To find the height of the tube due to the condensation, or the portion of the length of the tube which would have been occupied by the steam condensed, had no condensation occurred, we have $\frac{14{\cdot}7\,x}{772}=$ the number of pounds of water raised 1° by the heat transmuted into work; or putting T for the total heat, and t for the sensible temperature of the steam, $\frac{14{\cdot}7\,x}{772\,(\mathrm{T}-t)}=$ the number of pounds of water which would be converted into steam from, and at a temperature t, by, the same heat. If now, w represent the weight of a filament of water 1 foot high in the tube, we shall have, for the space occupied by the above weight of steam, in the state of water, $\frac{14{\cdot}7\,x}{772\,(\mathrm{T}-t)\,w}$ cubic feet. If V represent the specific volume of the steam, then will the above volume of water, as steam, occupy $\frac{14{\cdot}7\,\mathrm{V}\,x}{772\,(\mathrm{T}-t)\,w}$ feet of the length of the tube; whence

$$x=(\mathrm{V}-1)-\frac{14{\cdot}7\,\mathrm{V}\,x}{772\,(\mathrm{T}-t)\,w}.$$

or

$$x\left(1+\frac{14{\cdot}7\,\mathrm{V}}{773\,(\mathrm{T}-t)\,w}\right)=\mathrm{V}-1.$$

$$\therefore\quad x=\frac{\mathrm{V}-1}{1+\dfrac{14{\cdot}7\,\mathrm{V}}{772\,(\mathrm{T}-t)\,w}}\quad .\quad .\qquad (1)$$

To find V, let V_1 be the specific volume of steam, as compared with the original water at the temperature of 62° Fahr., and P represent the tension of the steam—above zero—in inches of mercury: then according to Fairbairn's empirical formula,

$$\mathrm{V}_1=25{\cdot}62+\frac{49513}{\mathrm{P}+{\cdot}72}.$$

Making $\mathrm{P}=30$, we get $\mathrm{V}_1=1637$; but we are seeking the specific volume of the steam as compared with the original water at the boiling point—212° in this case: to determine this, we make use of the relation that the specific volumes of the steam are inversely proportional to the volumes of a given weight of water at the two temperatures mentioned, thus;

$$\mathrm{V} : \mathrm{V}_1 : : 1{\cdot} : 1{\cdot}0365, \text{ *}$$

whence

$$\mathrm{V}=\frac{\mathrm{V}_1}{1{\cdot}0365}=\frac{1637}{1{\cdot}0365}=1579.$$

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* The volumes of a given weight of water, at 62° and 212°, are to each other as 1 : 10365,—very nearly.

Also at 212°, $w = \cdot 42$ of a pound; $\text{T} = 1178\cdot{}^\circ 6$; $t = 212^\circ$. Substituting these values in (1) we get

$$x = \frac{1579 - 1}{1 + \dfrac{14\cdot 7 \times 1579}{772(1178\cdot 6 - 212)\cdot 42}}$$

$$= 1469\cdot 3 \text{ feet.}$$

Hence the work done by ·42 of a pound of water, when converted into steam of one atmosphere, $= 14\cdot 7 \times 1469\cdot 3 = 21598\cdot 71$ lbs. ft.; consequently for the work done by a pound of water under the same circumstances, if evaporated in an hour and reduced to horse power, we have

$$\text{HP} = \frac{21598\cdot 71}{33000 \times 60 \times \cdot 42},$$

$$= \cdot 026. \qquad . \qquad . \qquad (2)$$

When steam is made under a pressure of 9 atmospheres, or $9 \times 14\cdot 7 = 132\cdot 3$ pounds per square inch, the work of a pound of water is found to be ·029 of a horse-power; but in securing the gain thus indicated, an additional expenditure of heat is required; so that when the proper reduction is made, we get ·0281 as the work accomplished by the same expenditure of heat or fuel that was required to develop the ·026 of a horse-power of (2). Now, assuming that the increments of work between one and nine atmospheres are equal—which, though not strictly true, is sufficiently near the truth for practical purposes—we get the following values. W being the work of 1 ℔. of steam per hour reduced to the standard of steam of one atmosphere.

Pressure in Atmospheres.	W.*	Remarks.
1	·026	Calculated.
2	·0261	Estimated.
3	·0263	"
4	·0266	"
5	·0269	"
6	·0272	"
7	·0275	"
8	·0279	"
9	·0281	Calculated.

In any case, to find the number of pounds of water required to be evaporated per horse-power (total), when the steam is used without

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[*] The numbers in this column, although strictly correct for the work of equal quantities of fuel only, may also be taken as representing the work of a pound of water, in each case, without appreciable error. The object here aimed at is more particularly to afford a means of comparison of the work done by equal weights of fuel.

expansion, and in a cylinder without clearance, it is only necessary to take the reciprocal of W, thus:

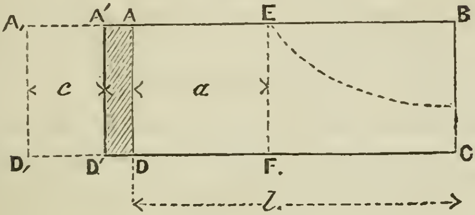
$$\text{cost} = \frac{1}{\text{W}}. \quad . \quad . \quad . \quad . \qquad (3)$$

$$\text{If W} = \cdot 026,\ \text{cost} = \frac{1}{\cdot 026} = 38 \cdot 446 \text{ pounds.}$$

We next pass to an estimate of the

Effect of Clearance.

Fig. 2.



In Fig. 2, let

l = length cf stroke = D C.

a = space traversed by the piston before the steam is cut off.

c = clearance = $\text{D}^1\ \text{D}_1$ = altitude of a cylinder whose diameter equals that of the cylinder, and whose volume equals the clearance space.

1 = unity = initial pressure of the steam above zero.

p_1 = mean pressure when the work per stroke is calculated independently of the clearance.

p_2 = mean pressure when the clearance is considered.

Then,

1. Neglecting the clearance, and calling the work done before the steam is cut off, unity, the work done during expansion will be represented by $hyp.\ log.\ \frac{l}{a}$; and the whole work per stroke

$$1 + hyp.\ log.\ \frac{l}{a}.$$

Dividing this result by the ratio of l to a, we get

$$p_1 = \frac{a}{l}\left[1 + hyp.\ log.\ \frac{l}{a}\right]. \quad . \quad . \qquad (4)$$

2. Considering the clearance as a part of the stroke, and calling the work done before the steam is cut off, unity, as before—but a different unit—we have for the work done during expansion in terms of this last unit,

$$hyp.\ log.\ \frac{l + c}{a + c}:$$

or, since the last unit is $\frac{a+c}{a}$ times as large as the first, this work, compared with the first, will be

$$\frac{a+c}{a}\ hyp.\ log.\ \frac{l+c}{a+c};$$

and, consequently, the work actually done by the steam during the entire stroke, in terms of the first unit, will be

$$1+\frac{a+c}{c}\ hyp.\ log.\ \frac{l+c}{a+c};$$

Dividing this by the ratio of l to a, as before, we get

$$p_2=\frac{a}{l}\left[1+\frac{a+c}{a},\ hyp.\ log.\ \frac{l+c}{a+c}\right]. \qquad . \qquad (5)$$

It is clear that there has been a *loss in steam*, due to the clearance space, while, at the same time, there has been a *gain in work*, due to the expansion of the clearance steam; but the loss is greater than the gain—hence an absolute loss equal to their difference.

The loss in steam, compared with the cylinder steam, is $\frac{c}{a}$; while the gain in work, compared with the work done before cutting off, is $\frac{p_2-p_1}{p_1}$.

The steam and work, in the first case being respectively equal to unity, and in the second case to $1+\frac{c}{a}$, and $1+\frac{p_2-p_1}{p_1}$, respectively; and assuming that the work done under any given conditions, should be proportional to the steam used, we have for the absolute loss of steam—or work,

$$\left(1+\frac{c}{a}\right)-\left(1+\frac{p_2-p_1}{p_1}\right),$$

$$=\frac{c}{a}-\frac{p_2-p_1}{p_1};$$

and for the loss in terms of, or compared with, the steam used in the second case,

$$\frac{\frac{c}{a}-\frac{p_2-p_1}{p_1}}{1+\frac{c}{a}}.$$

$$\therefore \quad \text{Loss}=\frac{c-\frac{a\,(p_2-p\ )}{p_1}}{a+c}. \qquad . \qquad . \qquad . \qquad (6)$$

If now we substitute in (6), for p_2 and p_1, their values in (4) and (5), we obtain, first;

$$\frac{p_2-p_1}{p_1}=\frac{\frac{a}{l}\left(1+\frac{a+c}{a}\,.\,hyp.\,log.\,\frac{l+c}{a+c}\right)-\frac{a}{l}\left(1+hyp.\,log.\,\frac{l}{a}\right),}{\frac{a}{l}\left(1+hyp.\,log.\,\frac{l}{a}\right)}$$

$$\frac{\frac{a+c}{a}\,hyp.\,log.\,\frac{l+c}{a+c}-hyp.\,log.\,\frac{l}{a}}{1+hyp.\,log.\,\frac{l}{a}}\;;$$

which in (6) gives, for the clearance, an absolute

$$\text{Loss}=\frac{c-\dfrac{a\left[\frac{a+c}{a}\,hyp.\,log.\,\frac{l+c}{a+c}-hyp.\,log.\,\frac{l}{a}\right]}{1+hyp.\,log.\,\frac{l}{a}}.}{a+c}$$

$$=\frac{c}{a+c}-\frac{\left[hyp.\,log.\,\frac{l+c}{a+c}-\frac{a\,.\,hyp.\,log.}{a+c}\,\frac{l}{a}\right]}{1+hyp.\,log.\,\frac{l}{a}}\,. \qquad (7)$$

By this formula, the following Table has been constructed; showing the losses due to clearance under the ordinary conditions of practice:

Cut-off.	Per cent. of Loss due to Clearance.			
	Clearance, 5 per cent.	Clearance 6 per cent.	Clearance 7 per cent.	Clearance 8 per cent.
$\frac{1}{10}$	20·90	23·65	26·48	28·93
$\frac{2}{10}$	13·60	16·68	18·73	20·90
$\frac{3}{10}$	11·25	13·19	15·03	16·18
$\frac{4}{10}$	9·40	11·01	12·60	14·18
$\frac{5}{10}$	8·09	9·57	10·98	12·21
$\frac{6}{10}$	7·20	8·47	9·76	10·97
$\frac{7}{10}$	6·47	7·59	8·79	9·84
$\frac{8}{10}$	5·79	6·84	7·85	8·98
$\frac{9}{10}$	5·21	6·23	7·19	8·16
$\frac{10}{10}$	4·76	5·66	6·54	7·69

Example.—Suppose it is found that, for a certain measure of expansion—without clearance—35 pounds of steam will be required per horse power per hour. Required the actual cost of a horse power when the clearance is 8 per cent. and the steam cut off at $\frac{2}{10}$.

We have

$$\text{Cost} = \frac{35 \times 100}{100 - 20{\cdot}90} = 44{\cdot}25 \text{ pounds:}$$

hence the absolute waste in this case will be 44·25 — 35 = 9·25 pounds of steam per horse power, per hour.

This reduction in the efficiency of steam, it will be observed, is in addition to the reduction due to condensation: taken together they probably amount to 40 per cent. of all the steam used; the exact amount, however, is at present unimportant; the immediate design being simply to direct the reader's attention to the progress being made in clearing away the mists which have so long obscured this subject.

It will be observed that if 40 per cent. of the steam is lost, or rather if only 60 per cent. of it is utilized, we shall realize, from a unit of steam, only $2{\cdot}609 \times {\cdot}6 = 1{\cdot}565$ units of work; that is, instead of realizing a gain of 1·609, we shall get only ·565; or about *one-third* the gain usually estimated for this measure of expansion.

We are now prepared to estimate the

Absolute Available Work, in a Unit of Steam or Water, for any measure of Expansion.

Let w_1 = the total work.

w = as before, the work due to a unit of steam, when used without expansion (2)

r = ratio of work theoretically due to a unit of steam, when used with any measure of expansion, to that done when the same steam is used without expansion—according to the theory as usually applied.

c_1 = loss due to clearance (7).

b = loss due to "blowing out."

Then retaining the previous notation we evidently have

$$\text{w}_1 = \text{w}r\,[(1 - c_1)\,(1 - b)]. \qquad (8)$$

in which $r = 1 + hyp.\ log.\ \frac{l}{a}$

Substituting now this value, together with that of c_1 already found (7) in (8), we get

$$\text{w}_1 = \text{w}\left[1 - \left[\frac{c}{a+c} - \frac{\left(hyp.log.\ \frac{l+c}{a+c} - \frac{a\ hyp.log.\ \frac{l}{a}}{a+c}\right)}{1 + hyp.\ log.\ \frac{l}{a}}\right]\right]\left(1 + hyp.\,log.\ \frac{l}{u}\right)(1-b)$$

$$= \text{w}\left[\frac{a\left(1 + hyp.\ log.\ \frac{l}{a}\right)}{a+c} + hyp.\ log.\ \frac{l+c}{a+c} - \frac{a\ hyp.\ log.\ \frac{l}{a}}{a+c}\right]\ [1-b]$$

$$\text{w}\left[\frac{a}{a+c} + hyp.\ log.\ \frac{l+c}{a+c}\right][1 - b]. \qquad (9)$$

The above expression would give the absolute work done by the steam, were it not for the fact that there is a loss of pressure between the boiler and the cylinder; that is, the initial cylinder pressure is always less than the boiler pressure; the difference being generally about three per cent. of the boiler pressure; although sometimes greater and sometimes slightly less. Hence if p be the boiler pressure above zero, in pounds, the initial cylinder pressure will be only $\cdot 97\,p$; while the average pressure during the stroke will be (4), without clearance,

$$\cdot 97\,p.\,\frac{a}{l}\Big[1 + hyp.\ log.\ \frac{l}{2}. \qquad . \qquad . \qquad (9^a)$$

or the work actually done will be ·97 of that due to the steam made in the boilers and (9) becomes,

$$w_1 = \cdot 97\,w\left[\frac{a}{a+c} + hyp.\ log.\ \frac{l+c}{a+c}\right][1-b.] \qquad . \quad (10)$$

The reciprocal of this, or $\frac{1}{w_1}$ will give the cost (c) of a horse-power per hour, in pounds of water evaporated, thus;

$$c = \frac{1}{\cdot 97\,w\left[\frac{a}{a+c} + hyp.\ log.\ \frac{l+c}{a+c}\right][1-b]}. \qquad . \quad (11)$$

But our object is to determine the net, or *effective* dynamic value of a unit of water—the surplus after deducting the constant prejudicial resistances of back-pressure, and the pressure due to the friction of the engine itself; which we will represent by v and f, respectively.

To correct the absolute dynamic value given by (9), it is only necessary to multiply that value by that fraction which expresses the value of the average effective pressure above zero, in terms of the boiler pressure: this value will obviously be (9^a)

$$\frac{\cdot 97\,p.\,\frac{a}{l}\left[1 + hyp.\ log.\ \frac{l}{a}\right] - [v+f]}{p}.$$

which multiplied by (9) gives for the net effective dynamical value w_1' of a pound of water.

$$w'_1 = \frac{w\left[\frac{a}{a+c} + hyp.\ log.\ \frac{l+c}{a+c}\right]\left[\cdot 97\,p.\,\frac{a}{l}\left(1 + hyp.\ log.\ \frac{l}{a}\right) - (v+f)\right][1-b]}{p.} \quad (12)$$

whence finally for the cost (c_1) of a net effective horse-power, we get

$$c_1 = \frac{1}{w_1{}^1};$$

or

$$c = \frac{p}{w\left[\frac{a}{a+c} + hyp.\ log.\ \frac{l+c}{a+c}\right]\left[\cdot 97\,p.\,\frac{a}{l}\left(1 + hyp.\ log.\ \frac{l}{a}\right) - (v+f)\right][1-b]} \quad (13)$$

An inspection of (13)—observing the manner in which p enters in both numerator and denominator—shows us that the value of c will be diminished by increasing p; by diminishing c, r, f, and b; and also that the slight increase of W, due to the increase of p, has the same tendency in a very slight degree.

We are now prepared to apply the formula to a practical

Example.—In the engines of the *Michigan*, under the conditions of the "Erie Experiments," $l = 96''$; $c = 5''\!\cdot\!57$; $r + f = 2\cdot7 + 2\cdot1 = 4\cdot8$; $p = 21 + 14\cdot7 = 35\cdot7$; and $\cdot97\ p = 34\cdot63$. To determine the cost of a net horse-power in pounds of water, when the steam is cut off at $\frac{4}{10}$ of the stroke from the beginning.

Making, for simplicity, $l = 100$, c becomes 5·8, and $a = 40$; then, putting $b = 0$, for fresh water, (13) becomes

$$c = \frac{35\cdot7}{\cdot026\left[\frac{40}{45\cdot8} + hyp.\,log.\frac{105\cdot8}{45\cdot8}\right]\left[34\cdot66 \times \cdot4\left(1 + hyp.\,log.\frac{100}{40}\right) - 4\cdot8\right]}$$

$$= \frac{35\cdot7}{\cdot026\ (\cdot873 + \cdot837)[34\cdot66 \times \cdot4\ (1 + \cdot916) - 4\cdot8]},$$

$$= \frac{35\cdot7}{\cdot026 \times 1\cdot71 \times 21\cdot76} = \frac{35\cdot7}{\cdot967} = 36\cdot92 \text{ pounds.} \quad . \qquad (14)$$

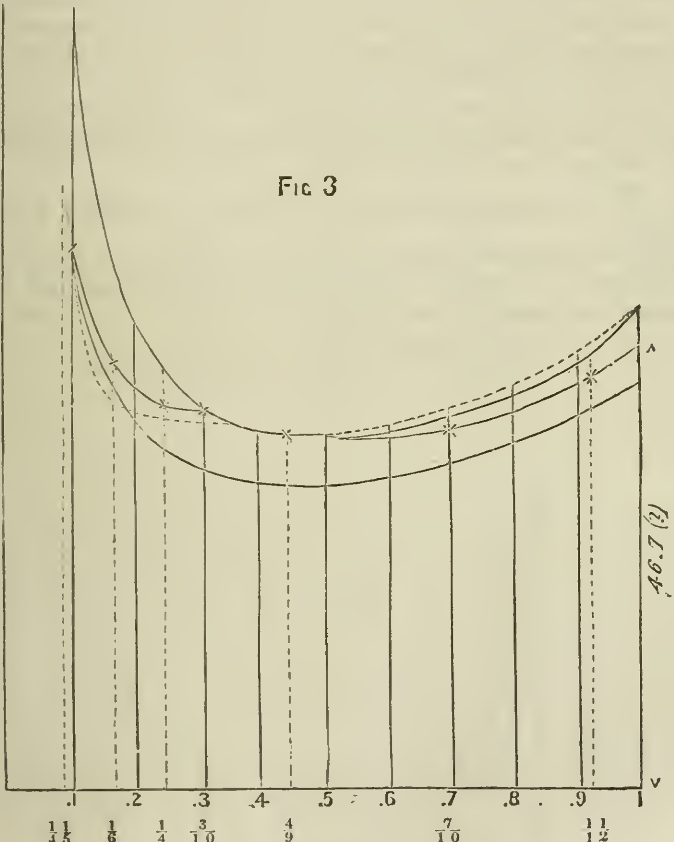
In the "Erie Experiments," the cost, when cutting off at $\frac{4}{9}$, was found to be 36·235 lbs.

Thus, in effect, the following Table embracing the conditions of the "Erie Experiments" with different pressures has been constructed; the steam under each of the assumed conditions of pressure, has been supposed to be cut off successively at $\frac{1}{10}$, $\frac{2}{10}$, $\frac{3}{10}$, - - - $\frac{10}{10}$ of the stroke, from the beginning.

Cut-off.	Cost of a Net Horse-power per hour in Pounds of Water.					
	$p = 2\cdot43$ Atmos. = 35·7 lbs.	$p = 3$ Atmos. = 44·1 lbs.	$p = 4$ Atmos. = 58·8 lbs.	$p = 5$ Atmos. = 73·5 lbs.	$p = 6$ Atmos. = 88·2 lbs.	$p = 9$ Atmos. = 132·3 lbs.
$\frac{1}{10}$	81·21	73·50	67·59	63·91	61·68	57·52
$\frac{2}{10}$	47·35	45·00	42·92	41·52	40·65	38·30
$\frac{3}{10}$	39·53	37·60	36·52	35·51	34·64	33·08
$\frac{4}{10}$	36 96	35·57	34·39	33·56	32·91	31·45
$\frac{5}{10}$	36·50	35·28	34·19	33·26	32·55	31·28
$\frac{6}{10}$	37·26	36·15	35·21	34·51	33·66	32·11
$\frac{7}{10}$	38·89	37·69	36·52	35·68	35·42	33·57
$\frac{8}{10}$	41·85	40·09	39·20	37·79	37·53	36·05
$\frac{9}{10}$	44·63	43·58	42·00	41·06	40·46	38·91
$\frac{10}{10}$	51·16	46·91	45·94	45·09	40·10	42·27

The diagram, Fig. 3, exhibits the law which governs the cost of net power, as determined by our formula (13)—and shows how nearly the results of theory, as we have applied it, agree with those of careful experiment. The ordinates of the curves represent the costs of a horse-power at the corresponding points of cutting off; the upper curve is constructed in accordance with the results given by (13), under the conditions of the "Erie Experiments;" the lower curve with the results for $p = 9$ atmospheres $= 132{\cdot}3$ pounds; and the middle curve with the results of the "Erie Experiments."* In the last case, the numbers represented by the ordinates which determine the curvature, are taken from line 29, Table I, facing page 100, in Chief Engineer Isherwood's "Experimental Researches in Steam Engineering;" the points of cutting off, in these "Experiments," were $\frac{11}{12}$, $\frac{7}{10}$, $\frac{4}{9}$, $\frac{3}{10}$, $\frac{1}{4}$, $\frac{1}{6}$ and $\frac{4}{45}$; and the corresponding costs, in pounds of steam per net horse-power, per hour, 42·971, 37·662, 36·135, 39·357, 39·202, 43·803 and 60·53 respectively.

FIG. 3



Vertical Scale, 1″ = 20 pounds.

It will be observed that, as regards the measure of expansion which insures the greatest economy, the results obtained by the use of our formula fully confirm the correctness of the conclusions of Chief Engineer Isherwood; it will also be observed that for points between $\frac{3}{10}$ and $\frac{7}{10}$ of the stroke, our theoretical results are nearly identical with the experimental results; and hence, since nearly all the points of cutting off employed in practice are embraced within the limits specified, it seems that we are justified in making the assumption that the formula will, in all practical cases, give results which may be relied upon as being practically correct. Again, since the point of cutting off which insures the maximum economy is the same for steam of 2·43 and 9 atmospheres, it may be assumed that it will be the same for all pressures between those limits.

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* The dotted curve has been added, representing the expenditure of *fuel* in the "Erie Experiments;" *i. e.*, its ordinates represent the costs of a horse-power, when the steam was cut off at the points at which they are erected.

It should be remarked also, that where the experimental results differ from those given by the formula, the experimental cost is *less* than the theoretical; hence if provision be made for the evaporation of the theoretical quantity of water for a given power, the development of the required power will always be realized.

Under the conditions upon which the "Erie Experiments" were made the formula indicates that the maximum gain, in net power, that can be realized from expansion is

$\frac{51\cdot16 - 36\cdot5}{56\cdot16} \times 100 = 26\cdot11$ per cent. of that developed when the steam is used without expansion.

According to the experimental results, the maximum gain due to expansion was probably (see Fig. 4)

$$\frac{46\cdot7 - 36\cdot235}{46\cdot7} \times 10 = 22\cdot41 \text{ per cent.}$$

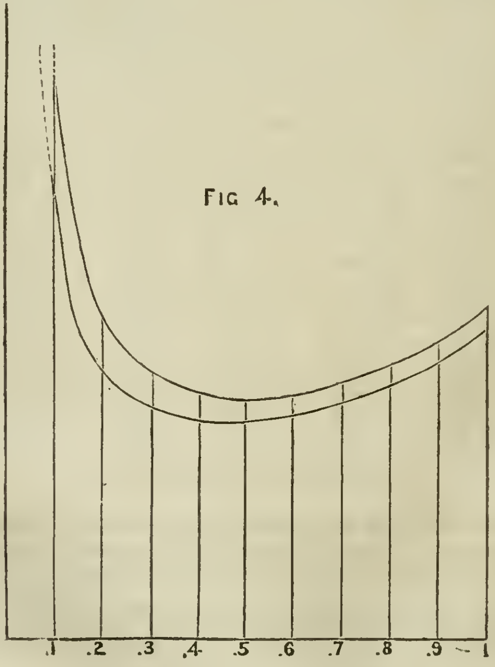


FIG 4.

Vertical Scale, 1″ = 30 *pounds.*

If we take ordinates of the dotted curve—of fuel—we shall obtain for the maximum saving in fuel, a result identical with that first found, from the results given by the formula (13). This is evident from the fact that the ordinates of the two curves, at full stroke, and at the point of minimum cost, are identical.

For the purpose of exhibiting the effect of clearance upon the measure of expansion due to the maximum economy, the following table—containing the costs for different values of *c*,—other things being the same as in the Erie Experiments—has been constructed: the numbers in the Table being obtained by substituting different values of *c*, in (13).

Cut-off.	Cost of a Horse-power.			
	$p=35{\cdot}7$ (1) c=2 per cent.	$p=35{\cdot}7$ (2) c=5·8 per cent.	$p=35{\cdot}7$ (3) c=8 per cent.	$\frac{(3)-(1)}{(3)}\times 100$
$\frac{1}{10}$	70·58	81·21	89·37	21·25
$\frac{2}{10}$	42·37	47·35	50·15	15·51
$\frac{3}{10}$	36·27	39·53	41·43	12·45
$\frac{4}{10}$	34·33	36·96	38·44	10·69
$\frac{5}{10}$	34·34	36·50	37·85	9·27
$\frac{6}{10}$	35·12	37·26	38·26	8·21
$\frac{7}{10}$	36·93	38·89	39·97	7·61
$\frac{8}{10}$	39·92	41·85	42·83	6·79
$\frac{9}{10}$	42·49	44·63	45·59	6·78
$\frac{10}{10}$	49·33	51·16	52·21	5·52

The results in columns (1) and (3) of the Table, are represented graphically in the diagram, Fig. 4, which shows that by diminishing the clearance space, the absolute cost of the power is materially reduced; while the measure of expansion which insures the best results, becomes slightly greater than when the clearance is larger; or, which is the same thing, the best point of cutting off is slightly nearer the beginning of the stroke, when the clearance is small.

If, in any case—having calculated the costs of a horse-power for different points of cutting off—it be desired to determine the costs for a different pressure, other things remaining the same, the labor may be considerably reduced by tabulating the values of the factor

$$\left[\frac{a}{a+c} + hyp.\ log. \frac{l+c}{a+c}\right],$$

in (13); which, it will be observed, is constant for all pressures.

The following Table contains the different values of this factor, for clearances, from 1 to 8 per cent. of the cylinder:

Cut-off.	$c = 1$ per cent.	$c = 2$ per cent.	$c = 3$ per cent.	$c = 4$ per cent.	$c = 5$ per cent.	$c = 6$ per cent.	$c = 7$ per cent.	$c = 8$ per cent.	$c = 5{\cdot}8$ per cent.
$\frac{1}{10}$	3·126	2·973	2.839	2·719	2·613	2·516	2·429	2·348	2·584
$\frac{2}{10}$	2·523	2·443	2·372	2·300	2·235	2·163	2·118	2·064	2·186
$\frac{3}{10}$	2·159	2·096	2·012	1·983	1·956	1·913	1·873	1·835	1·923
$\frac{4}{10}$	1·877	1·841	1·805	1·769	1·736	1·704	1·673	1·644	1·71
$\frac{5}{10}$	1·663	1·635	1·608	1·582	1·556	1·535	1·507	1·483	1·538
$\frac{6}{10}$	1·487	1·465	1·443	1·423	1·402	1·380	1·366	1·345	1·381
$\frac{7}{10}$	1 339	1·326	1·302	1·286	1·269	1·257	1·245	1·225	1·259
$\frac{8}{10}$	1·209	1·194	1·178	1·166	1·152	1·139	1·127	1·113	1·139
$\frac{9}{10}$	1·093	1·089	1·071	1·060	1·047	1·036	1·025	1·015	1·037
$\frac{10}{10}$	0·990	0·980	0·971	0·961	0·952	0·943	0·934	0 926	0·945

In conclusion it should be stated, that owing to the time and labor required, the numbers in the several tables that have been constructed in connexion with the discussion of this subject, have not been systematically verified; but in view of the great care which has been exercised in making the computations, to avoid errors, the writer does not hesitate to express the confident opinion, that whatever trifling errors may have crept in, have not sensibly affected the results; indeed, the uniform regularity of the curves constructed with the results—so far as curves have been constructed—is evidence of itself that appreciable errors have been avoided: however this may be, there seems to be no reasonable ground for doubt in regard to the conclusiveness of the proof of the correctness of Mr. Isherwood's "peculiar" theory—at least, so far as *economy* is concerned.

Naval Academy, Newport, R. I., April 30, 1864.

For the Journal of the Franklin Institute.

On the Steam-boiler Explosion at Cornelius & Baker's Works, Philadelphia, April 25, 1864. By John W. Nystrom.

It is proposed to explain how the steam-boiler explosion happened at the establishment of Cornelius & Baker, Cherry street below Ninth, Philadelphia, on the 25th of April, at 7½ o'clock in the morning. Being one of the jury, I felt at liberty to examine the case more carefully than I probably would have otherwise done.

The construction of the boilers is shown by the accompanying figs. 1 and 2. The lower cylinders are mud-drums, the middle ones the returning tubular boilers and the steam-drums above, with connexions *c*, *c*, *c*.

Fig. 1 is a section through *m*, *n*, fig. 2, and fig. 2 is a section through *o*, *p*, fig. 1.

The dimensions of the boilers are as follows:

	Diameter.	Length.	Cubic Feet.
Mud-drum, . . .	30 ins.	6 ft.	30
Returning tubular boiler, .	41 "	10 "	68
Steam-drum, . . .	36 "	$14\frac{1}{4}$ "	100
Connexions, c c c c, . .	13 "	8 ins.	2
Cubic contents of each boiler, ...			200
" two boilers, ...			400
" two steam-drums, . . .			100
Water in the two boilers, ...			300

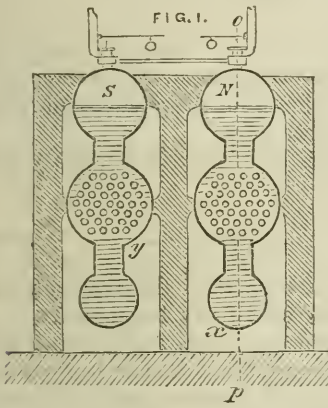


FIG. 1.

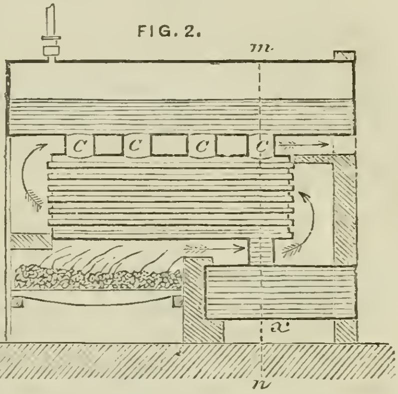


FIG. 2.

The working pressure of steam has averaged 80 lbs., but at the time of the explosion it was said to be only 65 lbs., which answers to a temperature of 315°. The total units of heat in both boilers was 4782660 in the steam and water, which, divided by Joule's equivalent, gives a concentrated force of 6200 horses. The heating surface of the boilers was 1200 sq. feet; fire-grate about 24 sq. feet. The heads of the steam and mud-drums are of cast iron $1\frac{3}{4}$ inch thick.

When steam boiler explosions take place, the enclosed volume of water W, of temperature T, will resolve itself into v volumes of boiling hot water of temperature t, and V volumes of temperature $6{\cdot}5\,t$ will be converted into steam.

Before. Explosion. After.

$$W\,T = v\,t + 6{\cdot}5Vt. \qquad (1)$$

and

$$W = v + V, \quad . \quad . \quad . \qquad (2)$$

of which

$$V = \frac{W\,(T - t)}{5{\cdot}5\,t}. \quad . \quad . \quad . \qquad (3)$$

In the case before us, we have been told that the pressure of steam was 65 lbs. to the square inch at the time of the explosion, which answers to a temperature of $T = 315° - 32° = 283°$. I have found by calculation that the two boilers contained $W = 300$ cubic feet of fresh water, the temperature of which is 212° when at boiling point, or $t = 180°$ above the freezing point. We have by the preceding formula (3),

$$V = \frac{300\,(283° - 180°)}{5{\cdot}5 \times 180°} = 31$$

cubic feet of water suddenly converted into steam, and mixed with $v = 269$ cubic feet of boiling hot water, which will form a destructive force of $H = \frac{31 \times 180° \times 5{\cdot}5 \times 62{\cdot}5}{772} = 2485$ horses.

The destructive energy of the $w = 100$ cubic feet of steam of $T = 283°$, and volumes co-efficient $k = 362$, will be only

$$H = \frac{wT}{2{\cdot}22\,k} = \frac{100 \times 283°}{2{\cdot}22 \times 362} = 34{\cdot}7 \text{ horses.}$$

The steam and water being in the form of a foam, not only acts with its elastic pressure, but with its momentum, the same as in Giffard's injector, where we know the force of the steam and water is much greater than the steam pressure.

The destructive momentum of an explosion may be correctly measured by

$$H = 80\,V = \frac{1{\cdot}7777\,W\,(T - t)}{t}, \text{ horses.} \qquad . \qquad . \quad (4)$$

The numerous and clear indications traced from the accident, enables me to give a very clear idea of how it actually happened.

I shall first explain it as if I had been on the spot and seen it with my own eyes, and then give the data upon which my argument is based. Should my reasoning not be clear, I hope to hear it discussed at the next monthly meeting of the Institute, so that, if possible, nothing will be left in mystery.

The mud-drum in the northern boiler was the first that gave way and caused the explosion. It burst near the middle about 4 ins. from the centre line, at *x*, figures 1 and 2; the numerous fragments of the mud-drum bruised and cut several holes in the shell of the tubular boiler above it, and also one hole in the south boiler at *y*, fig. 1.

The before calculated collective force in the boilers, is now suddenly relieved; its momentum finds the weakest part, first in the tubular boiler next to the bursted mud-drum, to be in the connexions *c c*, *c c*, where nearly half of the strength of the shell is cut away. It blew wide open the whole length, acted uniformly on the steam-drum above, threw the latter up with a velocity of 254 feet per second, or 173 miles per hour, into the air, at an angle of 47° 30′ south, by 7° 16′ east, struck the cornice of the main building at *a*, fig. 3, 60 ft. 4 ins. above, and 58 ft. 2 ins. horizontally from its home N. When it struck, it was at an angle of about 20° to the building, with the gauge-cock, or manhole end foremost. This obstruction caused it to deviate from its original course N *a c*, about 11° 16′, to *a b*, or south by 4° west, as shown by the figure; continued to a height of 272 feet, the vertex of the parabola. In the flight it made $1\frac{3}{10}$ turns in the length, and arrived with a velocity of 237 ft. per second, from an altitude of 51° 30′, in the Penn stable, Market street, just 1000 ft. from home. The drum seemed to have been horizontal and parallel with north and south, when it struck the roof of the stable, falling with the south end on a strong beam in the floor, broke through with the north end into the cellar, where it injured slightly one man, and killed a horse, rested in a position of about 55°. The time of flight was about 5·75 seconds. Had the steam drum continued unobstructed in its original course, N*ac*, figure 3, it would have fallen in Eighth street, between Market and Chestnut.

Let us now go back to the factory and see the next of the explosion.

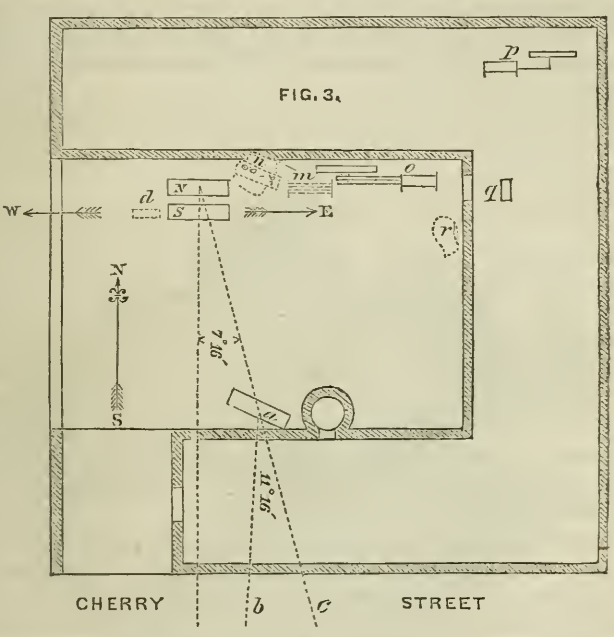


FIG. 3.

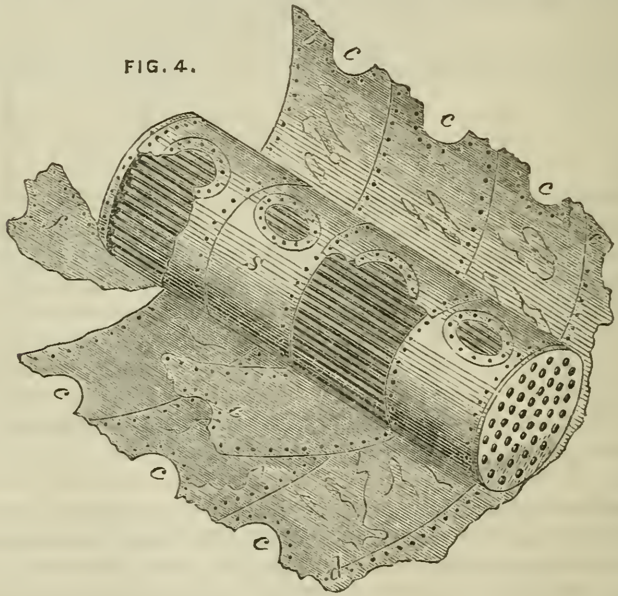
The tubular boiler burst open as described, the force of the steam and water acting in a general radiating direction below the fire on the grate in through the doorway, *q*, fig. 3, where it set fire to a box of wooden shavings, about 40 feet from the boilers; the tubes with the tube plates *m*, followed the fire about 20 feet towards the new steam engine, *o*. Three men were killed in this operation.

Now in reference to the boiler S, which, by the shock of the former explosion, was injured in the connexions *cccc*. The second ring *e*, fig. 4, gave way first, when the momentum in the steam and water broke the steam drum above it into two parts; blew the one east, nearly horizontal to *r*, where it damaged the stamping establishment. The fourth ring *f*, burst simultaneously to that of *e*; the steam and water acted to throw the west part of the steam drum straight up, while the longitudinal force acted to throw it horizontally due west; the resultant of the two forces caused the drum to ascend at an angle of 34°, and a velocity of 306 feet per second, or 208 miles per hour, attained an altitude of 228 feet, descended in a bottling establishment near to Twelfth street, with a velocity of about 291 feet per second, at an angle of 37° 10′, and 1350 feet from home s. The drum ricocheted a distance of about 52 feet from where it struck the floor. A man was killed by a piece of wood which the steam drum broke loose from the roof in the bottling establishment. The time of flight was about 5·4 seconds.

The distance, 1000 feet to the Penn stables; also, the 1350 feet to Twelfth street, was kindly furnished by Mr. D. H. Shedaker of the Department of Surveys.

Let us now return to the factory and see the balance of the explosion. The shell of the north boiler now being torn wide open and in flight, the south boiler followed and fell into the former at *n*, fig. 3, a distance of about 12 feet north-east from home, the position in which it was found after the explosion, and represented by figure 4.

FIG. 4.



The mud-drum of the south boiler was new, and not injured by the explosion: it moved backwards some 10 feet and rested in the position *d*, fig. 3. This mud-drum had been in use only a few months; the old one was found defective and therefore condemned.

The boilers have been in use about two years and a half.

I have in my possession samples of the exploded boiler and mud-drum, which it is my intention to deposit in the archives of the Franklin Institute.

It will be seen that the piece of iron from the mud-drum taken from the bottom where it first burst, is corroded down in one place to $\frac{1}{32}$ of an inch in thickness; several square feet in the bottom where it burst did not average more than $\frac{1}{16}$ of an inch; othewise the iron appeared to be of good quality. The inside of the drum shows plainly an excess of corrosion, by small holes eaten nearly through the metal.

In the Coroner's inquest, the engineer of the exploded boiler testified to having cleaned, sounded with a hammer, and with a light examined the inside of the exploded mud-drum, about three weeks before the accident, when he considered it in good condition.

The thickness of iron in the tubular boiler averaged a quarter of an inch, but in some places between the joints *c*, *c*, *c*, *c*, where it burst, it is as low as $\frac{3}{16}$ of an inch. The length of the boiler being 10 feet, of which four holes of 13 inches each or 52 inches is cut away, leaving only $120 - 52 = 68$ inches by $\frac{1}{4} = 17$ square inches to resist a pressure of $\frac{1}{2} \times 120 \times 42 \times 80 = 201600$ pounds, divided by 17 will be 11860 or 5·3 tons per square inch, which cannot be considered safe in this peculiar case.

When steam is fast generated it cannot rise sufficiently fast through the connexions to keep the water in contact with the upper part of the tubular boiler, subjected to a very high heat, which considerably reduces the strength of the metal.

The nature of those joints are such, that they require not only the very best quality of iron, but also the very first class workmen in turning the flanges, and with the greatest possible care, its strength will be but a small fraction of that of a solid sheet. Therefore, a boiler of nearly double the diameter, and with the same thickness of metal not cut, would have the same strength.

The strength of the mud-drum, until the explosion, had likely been assisted by the lateral strength of saturated scales and rust, filling up the small cavities in the iron. By some sudden change in temperature in the mud-drum, the scales and rust have likely cracked, and left the thin iron to resist the steam pressure, when explosion must necessarily take place, independent of height of water in the boiler.

It has been conjectured that the water had been low in the boiler when it exploded, but there is no positive indication of any iron having been overheated. A few tubes which had been torn from the boiler had indications of collapse, but no overheating; that collapse had likely been done by the momentum of explosion, (formula 4) which is quite a different force from that of the original steam pressure.

The boilers were fed by a No. 5 Giffard's injector, which was stopped a few minutes before the explosion took place; the engineer found by the gauge-cocks that there was plenty of water in the boilers.

Experienced engineers say that it is sometimes very difficult to observe the height of water in this kind of boilers; a glass water-gauge shows an oscillation of some 5 inches several times per second, accounted for by the imperfect circulation through the connexions *c*,*c*,*c*, which constitute one of their greatest defects.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

From the Lond. Mechanics' Magazine, July, 1863.

[Report of the Chief Engineer, July 28, 1863.]

During the past month there have been examined 324 engines and 450 boilers. Of the latter, 17 have been examined specially, 11 internally, 55 thoroughly, and 367 externally; in addition to which, 3 of these boilers have been tested by hydraulic pressure. The following defects have been found in the boilers examined :—Fracture, 8 (2 dangerous); corrosion, 16; safety-valves out of order, 9 (2 dangerous); water-gauges, ditto, 2; pressure-gauges, ditto, 8; blow-out apparatus, ditto, 37; fusible plugs, ditto, 2; furnaces out of shape, 4; over-pressure, 1 (dangerous); deficiency of water, 1 (dangerous); blistered plates, 3. Total, 110, 6 (dangerous). Boilers without glass water-gauges, 2; without blow-out taps, 38; without back-pressure valves, 41.

It is gratifying to find that the number of "thorough" examinations of boilers, though not high during the past month, is steadily increasing. During the month ending June 26th last, as many as 104 were made in addition to 12 "internal," and 10 "special"—making 126, independently of the ordinary "external" examinations. This number is a greater one than has been reached in any previous month.

Explosions.—One explosion has occurred during the last month, of a very fatal character, to an ordinary mill boiler of the two-flued "Lancashire" class, by which 10 persons were killed and four others injured. This boiler was not under the inspection of this Association.

The dimensions were as follows—length 30 ft., diameter of the shell nearly 7 ft. 6 ins., and that of the furnace tubes—which were parallel throughout, and not strengthened by any hoops or flanges—2 ft. 8 ins.; the thickness of the plates in the shell and tubes, seven-sixteenths, in the flat end plates half an inch, each of them being strengthened with three gusset stays secured with double angle-irons.

The longitudinal seams in the shell were not laid in line, but disposed so as to break joint. The age of the boiler was about two years. It had not been tested by hydraulic pressure.

The boiler had been fitted with a single lever safety-valve, the valve being enclosed in a box bonneted over, from which the waste steam escaped through a discharge pipe, carried through the wall of the boiler-house. It had also been fitted with a glass water gauge—a feed-check and back-pressure valve combined, fixed to the front end plate, a little below water level—a blow out or mud tap, and a steam pressure gauge, of the dial class; but the boiler had no tap for fixing an indicator so as to check the accuracy of the gauge, and ascertain the actual working pressure with the steam up.

The boiler was rent into so many fragments by the explosion, that it was completely destroyed, while considerable damage was also done to the surrounding property. Both the furnace tubes were torn away from the end plates, as well as separated into two pieces, dividing at one of the transverse seams at the middle of their length. Three of these lengths, weighing upwards of a ton each, were blown over a row of cottages, one alighting on the first floor of a dwelling beyond, having broken through the roof in its fall, the other two lengths falling at intermediate distances between these two rows of buildings; while the fourth fell on a cottage, carrying in the roof. The safety-valve weight, which was a ball of about 8 ins. diameter, was shot upwards, and on its fall, broke through a roof of a third cottage. The shell of the boiler had been torn up into so many small pieces that it was difficult to trace the course of the rents, and to determine where they had first commenced; but it may be remarked, that one of them ran through the manhole, which was not strengthened as it should have been by a substantial mouth-piece. These fragments of the shell lay scattered near the original seat of the boiler, while some were buried under the ruins. The end wall of the mill was blown down, and the various floors laid open, while the engine was completely buried in the *debris.* The chimney was gashed by a large rent, running up it for half its height, and stood tottering over the old seat of the boiler, so that approach was dangerous; while the ground surrounding was covered with bricks, and the ruins of the injured buildings. But it is difficult to convey an adequate idea of the ruin that had been produced.

The cause of the explosion has twice been investigated before a coroner's jury, conducted in each instance by a different coroner on account of the localities in which the deaths occurred. Scientific evidence was given in each case. At the first trial, one scientific witness gave it as his opinion that there were no traces of there having been any deficiency of water, but that the safety-valve had been deranged and inoperative; while at the second trial, another scientific witness thought that the safety-valve had worked quite "glibly," and that shortness of water had caused the explosion. A verdict of accidental death was brought in in each instance.

With regard to the explosion having arisen from shortness of water, it was given in evidence at the inquest that the gauge-glass was blown through, and plenty of water observed shortly before the explosion happened: the correctness of which was borne out by subsequent examination, since the flues were found to be coated with incrustation which overheating, had it occurred, would have disturbed; while, in addition, the furnace-crowns could not have retained their shape as they did, had the water been low.

As to the explosion being due to excessive pressure consequent upon the defect of the safety-valve, it is true that the spindle of the valve was bent, but it is a matter of opinion whether this did not become so subsequently to the explosion, and was its consequence rather than its cause. At the time of making my own examination, which was done immediately on the explosion being reported, there was no opportunity of seeing the safety-valve, since it was in the hands of the jury who were then engaged on the inquest—but I am informed, on good authority, that the injury was such as could not have occurred to the valve in regular work, while it was known to have been previously in good order.

But, apart from the condition of the safety-valve, there are other considerations affecting the view that the explosion was due to excessive pressure alone. There was no evidence to prove that the pressure exceeded 60 lbs. per square inch, and neither that nor twice the amount would have rent the shell had the material and workmanship been good; while from the fact that the flue-tubes were not collapsed, and the shell was rent into fragments—although the latter should have resisted twice the strain of the former—it is clear that the explosion did not result from simple over pressure.

It cannot be doubted that the plates were of very bad quality, one of them in the shell, situated at the top of the external flue had fractured through the solid when the boiler was at work a few months since, while it is reported that one of the scientific witnesses who gave evidence at the inquest stated—that merely with the blow of a brick he had broken off a piece of plate about 15 or 16 sq. ins. in area, and seven-sixteenths in thickness. The cast iron nature of the plates rendered them less adapted to withstand the tensile strain of the shell than the compressive one of the furnace tubes which therefore accounts for the shell having been broken to pieces, while the furnace tubes were uninjured, except by the effects of the explosion itself.

Such is the simple cause of this explosion. The evidence given at the inquest, as well as the examination of the furnace-crowns, forbids the conclusion that the explosion was caused by deficiency of water; while, further, the fact that the shell, which should have been stronger than the tubes, rent into a number of small pieces while the tubes did not collapse, shows that the explosion was not due to excessive pressure, but to the defective quality of the plates of which the boiler, on examination, was found to have been made.

In conclusion, the attention of our members may be specially called to the following points:—

The contradictory nature of the evidence too frequently admitted at coroner's inquests as to the cause of boiler explosions, of which the case above is an illustration.

The short-sighted economy of purchasing low-priced boilers, erroneously termed "cheap" which leaves the maker no alternative but to use plates of inferior quality, a practice not only detrimental to the interests of the steam user, but also unfair to the honest boiler-maker.

The importance of having all boilers thoroughly tested with hydraulic pressure; this had never been done with the boiler under consideration; had the test been applied, there can be little doubt that the inferior quality of the plates would have been detected, and the explosion prevented.

[Report for August, 1863.]

From the Lond. Mechanics' Magazine, August, 1863.

During the past month there have been examined 313 engines and 401 boilers. Of the latter 6 have been examined specially, 9 internally, 45 thoroughly, and 341 externally, in addition to which, two of these boilers have been tested by hydraulic pressure. The following defects have been found in the boilers examined:—Fracture, 5 (1 dangerous);

corrosion, 14; safety-valves out of order, 7; water-gauges ditto, 9 (1 dangerous); pressure-gauges, ditto, 15; feed apparatus, ditto, 2; blow-out apparatus, ditto, 27; fusible plugs, ditto, 1; furnaces out of shape, 4 (1 dangerous); over-pressure, 3; deficiency of water, 1 (dangerous). Total, 88 (4 dangerous). Boilers without glass water-gauges, 3; without blow-out apparatus, 16; without back-pressure valves, 25. Nine explosions have occurred during the past month, from which 5 persons have been killed and 3 others injured. Not one of the boilers in question was under the inspection of this Association. A personal examination of three of these has been made subsequently to the explosion, while in the remaining cases this was prevented by distance. In one case the cause of the explosion was simply the dilapidated condition of the boiler; it had repeatedly been found to leak at the back end, both at the last plate at the bottom, as well as at the flat end one, and had in consequence, been temporarily repaired, from time to time, with bolted patches. Patches, when necessary, should always be riveted on, no reliance can be placed upon those merely bolted. At the time of the explosion there were three of these patches on the boiler, within 12 ins. of one another. The surrounding plate at length became so eaten away by continual leakage, that it was reduced in places to one-eighth of an inch in thickness, and in others to that of a sheet of brown paper, from which rupture ensued, underneath the boiler, at the back end, immediately over the mid-feather; the mid-feather, no doubt, accelerating the corrosion, by ponding the water, and holding it in contact with the plate, at the same time that it concealed the full extent of the injury. Competent inspection could not have failed to detect the dangerous condition of the boiler. The report concludes with suggestions as to the best mode of setting boilers.

[Report, September 29th, 1863.]

From the London Mechanics' Magazine, Oct., 1863.

During the past month there have been examined 319 engines and 471 boilers. Of the latter, 4 have been examined specially, 4 internally, 49 thoroughly, and 414 externally, in addition to which 1 of these boilers has been tested by hydraulic pressure. The following defects have been found in the boilers examined:—Fracture, 5 (2 dangerous); corrosion, 16; safety-valves out of order, 2; water-gauges ditto, 20 (1 dangerous); pressure-gauges ditto, 10; feed apparatus ditto, 2 (1 dangerous); blow-out apparatus ditto, 14; fusible plugs ditto, 8; furnaces out of shape, 1. Total, 78 (4 dangerous). Boilers without glass water-gauges, 3; without blow-out apparatus, 21; without back-pressure valves, 17.

Explosions.—Three boilers, not under the inspection of this Association, have exploded during the past month, from which four persons have been killed and three others injured. One of the boilers has been personally examined since the explosion, while this was prevented in the other cases by distance.

The first explosion, from which one person was killed and two others injured, happened to an agricultural boiler while at work at a farm, driving a thrashing machine. It was not under the inspection of this Association.

The boiler which was about twelve years old, was a portable one, of the multitubular locomotive type, and was worked at a pressure of about 40 lbs. per square inch. It was stated at the inquest that the safety-valve worked freely, and that there was a due supply of water the explosion being attributed entirely to a defective plate in the fire-box, which had been eaten away by corrosion, until reduced to one-sixteenth of an inch in thickness. This plate had been previously repaired, and at the time of the explosion was cracked through, and though it had for some days been leaking in consequence, had yet been worked on in that state.

The owner of the boiler was brought in guilty of manslaughter, the jury adding that they thought the appointment of a government inspector to be highly necessary.

The second occurred to a new balloon or hay-stack boiler during the operation of testing at the maker's yard. It had not been constructed for raising steam for purposes of power, but was intended to be used as a chemical evaporating pan or still. It was not under the inspection of this Association.

The boiler—as another of similar construction and dimensions had recently been—was proved with steam, in order to test the tightness of the joints. The pressure, which appears to have been about 50 lbs., being raised, not by generating the steam within it by means of a fire underneath, but by communication with another boiler in work, to which it was temporarily connected for the purpose. Some five or six men were engaged upon it in caulking the seams at the time of the explosion, four of whom were blown to a distance from it; one of them, who was killed upon the spot, being thrown upon the roof of a house about 40 yards off, while two others died subsequently from the injuries received. The boiler was laid upon its side during the test, and gave way at the bottom, which was blown out entire, and thrown upon a roof about 10 ft. high and 13 yards distant, the shell rearing up on its dome end, and remaining in that position supported by a shear leg.

Its dimensions were as follows:—The height was 11 ft., the dome, from the appearance of which the term balloon boiler probably arose, was 9 ft. 3 ins. in diameter, and the base 8 ft. 9 ins., while the cylindrical sides were drawn in to 7 ft. 9 ins. in the waist. The bottom, like that of a bottle, was concave, as is usual in these boilers, and rose 18 ins. at the centre, in order to enable it to resist the downward pressure, being connected to the sides of the shell by a 3-in. angle iron, half an inch thick. The plates, which were from seven-sixteenths to half an inch in thickness throughout, were some of them second-hand, those forming the dome being new, the remainder having been used in a boiler before. They had, however, been worked to their new shape, their edges sheared, and the holes punched afresh. The rivets, which were three-quarters of an inch in diameter, were centred from $1\frac{7}{8}$ ins. to 2 ins. apart, and the general workmanship appeared satisfactory.

The rent between the bottom and sides of the boiler had occurred, for the most part, at the lower ring-seam of rivets in the cylindrical portion of the shell but not altogether so, some portion of the rent running through the rivet holes at the outer edge of the circular bottom plate; while, from the upward position in which the latter was thrown by the explosion, it appeared most probable that the rent had commenced at that part of the boiler which lay nearest the ground at the time of the explosion, and thus from the position of the fractures, must have started at the cylindrical part, and not at the bottom plate. The reason of this is not very apparent—since there were two other ring-seams of rivets in the cylindrical portion of the boiler which were quite uninjured, although subjected to the same direct pull, which, after allowing for the rivet holes, did not amount to two tons per square inch of metal, added to which, the longitudinal strain thrown upon these circular seams in cylindrical boilers, is only half of that which occurs in the transverse direction, so that it is difficult to account for the rent selecting the lower ring-seam of rivets, or indeed for its occurring at all.

Whether, however, the rent in question was due to some undetected flaw in the iron, or whether the plates had been crippled in being brought together, which is too frequently the case, or whether an unfair strain was caused by the springing of the bottom plate, which, although arched eighteen inches in the centre, was yet flat as compared with the hemispherical top; or whatever may have been the cause that started the rent, one thing is certain, that the application of the hydraulic test would have detected and exposed the weakness, prevented the explosion, and saved the three lives. It is, therefore, again earnestly recommended that in all similar cases, this precaution should be adopted; and it is trusted that it will not be necessary for any further additions to be made to the list of fatal explosions—already sadly too long—before the adoption of the simple and inexpensive hydraulic test becomes universal.

A Magnetic Discovery.

From the London Mechanics' Magazine, Feb., 1864.

Dr. A. von Waltenhofen has communicated to *Dingler's Polytechnisches Journal*, an account of a curious magnetic discovery which he has recently made. It is a well-known fact that the magnetism of an electro-magnet does not entirely disappear with the cessation of the magnetizing current. Dr. A. von Waltenhofen has, however, observed that the amount of this residual magnetism, as it is called, is dependent upon the manner in which the current is interrupted. If this interruption takes place suddenly, the residual magnetism is much less than when it takes place gradually. A still more interesting circumstance has been observed by him—viz: that the residual magnetism obtained by suddenly breaking a very strong current, is sometimes of an opposite nature to that previously existing in the electro-magnet. This fact, which he has hitherto only noticed in very soft iron, is of great interest, inasmuch as it furnishes a new and simple proof that magnetism is not caused by the separation of two fluids, but by the motion of magnetic molecules, to which is opposed a certain amount of frictional resistance. With much ingenuity he compares the state of each magnetic molecule of the electro-magnet to that of a spring which is bent back. If the spring be suddenly released, it will return very nearly to its original position, or even go beyond it. On the other hand, if it be released gradually, it will stop at a point still further removed from its original position.

Voltaic Electricity—New Caloric Battery.

From the Lond. Mechanics' Magizine, Feb., 1864.

At the last meeting of the Inventors' Institute, (Capt. J. Selwyn, R.N., in the chair,) Mr. James Dickson read an interesting paper "On Certain Inventions for insuring the Economical and Efficient Production of Voltaic Electricity for Lighting Streets and other Purposes." The object of the paper was to explain the means by which electricity could be readily and economically produced. The history of voltaic electricity was carefully traced from the time of Volta, from whom this form of electricity took its name, to the present time, special mention being made of Grove's, Smee's, the Maynooth and other batteries which from time to time, have been looked upon as vast improvements upon then existing apparatus. The theories of Mayer and Joule were referred to, as well as the researches of Prefessor Tyndall, whose "Heat as a Mode of Motion" contains so much valuable information upon the subject. He considered that the rapidity of the vibration of the atoms in a conductor was exactly in proportion to its conducting power, and explained that, whilst a battery was producing light and heat, less material was being consumed than when the battery poles are directly connected with each other. Mr. Dickson's battery was described as one of the hot-class—the sulphuric acid was heated to 600 degs. Fahr. He claims by his mode of applying heat to be able to use iron and other cheap metals, instead of the dear ones—zinc, copper, &c. The relative mobility of the atoms of an electrolite determined, he considered its force rather than its specific gravity. When oil of vitriol was heated to 350 degs. Fahr. only, the electric action is less powerful than when heated to 600 degs. Fahr., probably owing to the waves being less rapid. With the necessary percolating apparatus he was convinced that his battery would be successful for lighthouse purposes. He considered 15 to 20 of his cells equal to 20 to 22 cells; 3 of his cells are not equal to 2 of the nitric acid cells, but the increment in his battery was greater. Grove's battery cost 1s. 5d. to produce the same amount of electricity as that produced for 10¼d. by Dickson's. Comparing the lightning powers, 11¼d. with the caloric battery, will produce the same amount of light as 1s. 5d. by Grove's. He declared that the sulphur liberated at the negative poles could be reconverted into sulphuric acid to the extent of 19-20ths. The oil of vitriol, during the working of the battery, becomes combined with water, but the acid is easily and cheaply reconcentrated. In Smee's, Daniel's, and Grove's battery, the sulphate of zinc cannot be recovered, whilst in his caloric battery the recovery was not difficult.—The chairman expressed the fear that the invention promised so much that he was no more likely to perform it than to obtain perpetual motion; indeed, if the invention were not overstated, they would certainly be nearer perpetual motion than they had ever been before.—Mr. Varley suggested that as the principle feature in the invention appeared to be the heating of the matrials, it was not impossible that it might be as great a step in advance as the introduction of the hot-blast in the manufacture of iron: this of course remained to be seen.—The new light will be exhibited at the meeting, on February 18, when an opputunity will be afforded for examining the caloric battery in operation.

Explosion of a Steam-Boiler in the establishment of Merrick & Sons, Philadelphia—Verdict of the Coroner's Jury.

In examining into the cause of the late disastrous explosion, we find the following facts presented to us:

A boiler built upon a certain plan, had been in use in the establishment for seven years; it had proved economical in its consumption of fuel, and occupied but little space; as, however, it had been in constant use for the above number of years, it was deemed advisable by the firm to construct a new boiler, which might either work alternately with the old one, or take its place in supplying them with steam.

The older boiler had never given them any trouble, except in one part called the water-legs, in the back part of which, that is, back of the bridge wall, sediment had collected, and caused the iron to rust out.

Satisfied with the efficiency of the first boiler, the new one was designed upon the same general plan as the old one, every precaution being taken (as was supposed) to make it strong and durable: and to obviate the trouble arising from the accumulation of sediment in the water-legs, a modification was made in that part.

The second boiler being completed, was tested and found to leak in a certain place. This leak was mended, and after several trials the boiler was pronounced tight and ready for use. The boiler was then fired, and furnished steam for the establishment, at first in connexion with the old one, and finally for several days by itself.

On the morning of April sixth, the day of the explosion, the leak was found to have re-opened to such an extent as to cause uneasiness as to the water-supply, on the part of the foreman of the establishment, Mr. Danby, who gave orders to have steam raised in the old boiler, and to draw the fires in the new one.

While these orders were being carried out the explosion occurred.

Examining into the competency of the man in charge of the boiler, the conviction is forced upon us that he was well qualified for his situation, and was a good, careful man.

Abundant testimony has also been given to prove that the pressure of steam at, or immediately before, the explosion was not more than fifty-seven pounds, and that the boiler was well supplied with water.

The usual theories of explosions, namely low-water, sudden priming or forcing up of water into the heated, steam, &c., seem not to be applicable to the present case, and we are forced into a consideration of the form of this boiler and the possible existence of some weak point which did not exist in the old one.

This boiler is of form similar to those used on steam vessels, and having flat sides and an arched roof, is mainly dependent upon stays for its strength. (See Plate IV.)

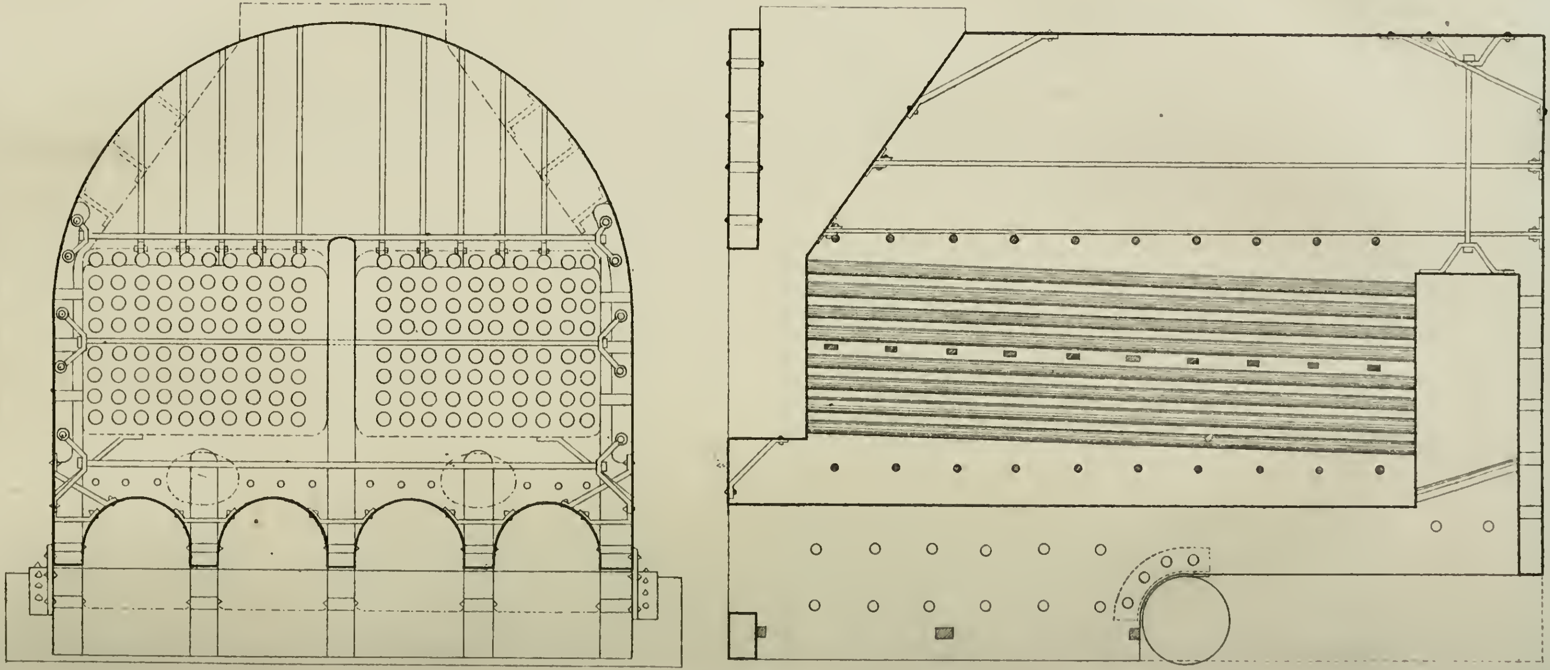
The sides seem to have been abundantly supplied with these stays, and to have been but little injured by the explosion, while the whole of the bottom part, containing the furnaces has been violently rent from the sides, and projected in one direction, while the main part of the boiler was thrown in an opposite direction.

The bottom or furnace part of the boiler consists of a series of arched passages used as furnaces; said passages being twenty-one inches wide, semicircular on the top or crown, and stayed from one to the other by a series of iron braces. The parts between the arches are what have been termed water-legs. These water-legs on the old boiler extended from the front to the back of the boiler, thus forming powerful beams, thirty inches in depth, to resist the pressure of the steam tending to push the bottom out of the boiler. At the front they are connected by a water-space below the doors of the furnaces, and at the back by a water space extending to the bottom; and thus were firmly united, and formed as it were, a floor supported by beams about thirty inches deep, five inches wide, and only thirteen feet long, which were, moreover, tied together top and bottom, at both ends to prevent their spreading at the bottom from the pressure above; a form admirably adapted to carry the load placed upon it.

In the new boiler we find that the beams upon which depend the stability of the bottom, were not continued from end to end of a uniform depth, but by the cutting off of the part which was filled with sediment in the old boiler, have been reduced to a depth from the crown to the bottom of but thirteen inches for a distance of one-half of their entire length. Hence the floor-beams, as it were, are reduced to less than one-half the depth of those in the old boiler, namely from thirty inches to thirteen inches in the centre of the boiler bottom. The most valuable part of the beams having been removed by this operation, and the main support of the crown sheets taken away, no additional stays were put in to compensate for this weakness.

Without going into any calculation of the strength of the floor of the boiler, we see that it is not half as strong as the old one, and has yielded under a pressure of only fifty-seven pounds per square inch.

That the yield took place at this part, in the very centre of the boiler bottom, is manifested by the leak, which persistently appeared at this very part, where a rupture should have begun if the floor was too weak. This leak was mended from time to time, but, on the day of the explosion, had increased to such an extent as to endanger the water-supply, and to cause the order to be given for discontinuing the use of this boiler, unfortunately too late, although this order was promptly given, and was in course of execution.



Scale 1 inch to 3 feet.

Your jury believe that the diminished strength which resulted from the change escaped the notice of the constructors, and that the making of the new boiler and its management afterwards, were designed as precautionary measures, and were intended to avoid the very disaster which has fallen upon them and the unfortunate victims.

Coleman Sellers,	Jonathan Chapman,
Henry Morton,	John F. Frazer,
John Agnew,	Gavin H. Woodward.

Substitutes for Gutta-Percha.

From the Journal of the Society of Arts, No. 589.

The following particulars in reference to the *Balata*, stated in last week's *Journal* to have been sent over by Sir W. Holmes from British Guiana, have been received. Sir William Holmes says:—

By the last mail I received your "Subjects for Premiums" for the session of 1863-64.

My object in addressing you is to advise, that by this packet I forward a box containing samples of *Balata* in its milky state, and also dried or coagulated. I entertain the hope that these samples fully meet the requirements of the 77th Section of your list of premiums, in reference to a substitute for india-rubber or gutta-percha, but I also trust it will be found more valuable than india-rubber or gutta-percha by themselves, possessing much of the elasticity of the one and the ductility of the other, whilst it requires a much higher temperature to melt or soften it.

After these preliminary observations, I must go somewhat into detail. I was Commissioner, representing the colony of British Guiana, at the International Exhibition of 1862. Amongst the varied contributions from the colony was a morsel of the dried milk of the bullet-tree (*Sapota Mulleri-Miq?*); it weighed perhaps half a pound. Amongst the numerous individuals who visited the Guiana department was Mr. Chas. Hancock, who is well known in the gutta-percha trade. This gentleman was struck with the appearance of the specimen, and obtained a portion for experiment; he reported favorably as to its utility and value, a result most gratifying to me, as I had received adverse opinions from less experienced persons. This happened, I think, in July, 1862. From that time to the present I have been engaged in investigations how to produce the material cheaply, and how to dry or coagulate it rapidly. In both particulars I believe I have succeeded so far as to warrant the importation of steam machinery to be applied to its extraction, and by a fortunate accident I have discovered how to dry or coagulate it, preserving the characteristic of elasticity at a single operation, by the addition of a simple ingredient not very costly.

The samples forwarded consist of—1st, a bottle of milk as extracted from the tree by tapping; 2d, of lumps or cakes, weighing together five lbs., of this milk prepared for the market; and 3d, some balls to show, by the result of the process discovered by me, that this material is nearly as elastic as india-rubber; indeed, as far as I can judge, *Balata* cannot be rivalled by either that material or gutta-percha, possessing, as I before stated, much of the elasticity of the one and the ductility of the other, without the intractability of india-rubber or the brittleness and friability of gutta-percha. Amongst the useful properties possessed by *Balata*, I believe the fresh milk of the bullet-tree to be the best water-proofing material yet discovered, and further that, *Balata*, as prepared by me, will supply the great want of the day, as a good insulating medium for telegraphic purposes.

The bullet-tree is a magnificent timber tree, often squaring 30 to 40 inches, and is much used, especially in Berbice, for building purposes. The milk, when quite fresh, is so bland that it is sometimes used as a substitute for cow's milk, and the fruit is delicious.

The bullet-tree abounds in many districts of the colony; indeed I may say, throughout this part of South America, and I trust that *Balata* may ere long be added as an important item to the exports of the colony, and tend to prove that the International Exhibition of 1862 has, in this instance, also been productive of practically useful results, not only to this community, but to the interests of science and art generally.

I annex a letter from the Honorable William Walker, Government Secretary of the colony, and Chairman of the Corresponding Committee of the Royal Agricultural and Commercial Society of British Guiana, which is affiliated to the Society of Arts, in order to fulfil the conditions specified in the prospectus.

Substitute for Gutta-Percha.

From the London Artizan, March, 1864.

At a meeting of the French Academy of Sciences, M. Serres gave an account of the *Valata*, a shrub which abounds in Guiana, and affords a juice which he asserts is superior, for many purposes, to gutta-percha, but especially as an insulating material for enveloping telegraphic wires. The milk or juice is drinkable, and used by the natives with coffee. It coagulates quickly when exposed to the air, and almost immediately when precipitated by alcohol, which also dissolves the resin of the *Valata* juice. All the articles made with gutta-percha can be made with the sap of the *Valata*, and it has no disagreeable smell. When worked up it becomes as supple as cloth, and more flexible than gutta-percha. M. Serres exhibited a number of articles manufactured of *Valata* milk. Up to the present time it seems, from M. Serres' account, not to have become an article of commercial export.

Method of Making Cast Steel. By M. GALY CAZALAT.

From the London Practical Mechanic's Journal, Sept., 1863.

This metallurgist has communicated to the Academy of Sciences at Paris, that by passing through fused cast iron at 1400° centigrade, numerous capillary jets of superheated steam, he is enabled rapidly and fully to remove the carbon which is taken up by the hydrogen of the decomposed water, and also to burn out, by the oxygen liberated from the water, the silicon, while the sulphur, the arsenic, and even the phosphorus are removed in combination with the hydrogen. Should this prove to be so, we may have an auxiliary to Bessemer's method, at least, and possibly even a substitute for it. We await, however, fuller details and confirmations.

Preservation of Corn from Fermentation.

From the London Mechanics' Magazine, January, 1864.

An experiment was lately made in Paris for the preservation of corn from fermentation and the attack of insects by enclosing it in a metal vessel and exhausting the air. The experiment was made in the presence of 40 persons, and succeeded perfectly. Ten hectolitres of wheat were placed in a metal vessel, and the air was exhausted. The vessel was opened after 15 days, and the weevils, which were seen quite lively when the wheat was placed in the vessel, had quitted their cells and were dead. They were warmed but did not stir. Being placed on white paper, they were crushed and reduced to powder, without leaving any stain on the paper. From various experiments made on wheat under glass, it was found that the weevil retains life longer than any other insect when deprived of air.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, April 21, 1864.

William Sellers, President, in the chair.

John H. Towne, Vice President, }
John F. Frazer, Treasurer, } present.

Washington Jones, Recording Secretary.

The minutes of the last meeting were read and approved.

Letters were read from the Liverpool Literary and Philosophical Society, Liverpool, England; the Literary and Historical Society of Quebec, Canada; and Hon. John D. Watson, Pennsylvania Legislature, Harrisburg, Pennsylvania.

Donations to the Library were received from the Royal Astronomical Society, and the Chemical Society of London; the Literary and Historical Society, Quebec, and the Canadian Institute of Toronto, Canada; H. A. Wise, Esq., U. S. N., Chief of Bureau of Ordnance, Navy Department, Charles Coloné, Esq., and F. Emmerick, Esq., Washington, D. C; the Executive Committee of the Union Relief Association of Baltimore, Md.; James Hall, Esq., Albany, and H. Whitall, Esq., City of New York; Young Men's Mercantile Library Association, Cincinnati, Ohio; Prof. John C. Cresson, Prof. John F. Frazer, H. P. M. Birkinbine, Esq., Dr. Thomas S. Kirkbride, and the Directors of the Girard College, Philadelphia.

Donation to the Cabinet of Models was received from Philip Griffith, Esq , Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's report for March was read.

The Board of Managers and Standing Committees reported their minutes.

The President reported his letter to the Hon. Secretary of the Navy, transmitting the names of the committee appointed to act on the joint committee on the expansion of steam; also his letters to Hon. Jacob Ridgeway of the Senate, and to Hon. John D. Watson, House of Representatives, Pennsylvania, transmitting the petition of the Institute, asking for amendments to the Charter, which were read.

Candidates for membership in the Institute (7) were proposed, and the candidates proposed at the last meeting (10) were duly elected.

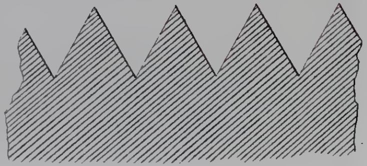
William Sellers, Esq., President of the Institute, read the following paper on a System of Screw Threads and Nuts:

The importance of a uniform system of screw threads and nuts is so generally acknowledged by the engineering profession, that it needs no argument to set forth its advantages; and in offering any plan for their acceptance, it remains only to demonstrate its practicability and its superiority over any of the numerous special proportions now used by the different manufacturers. In this country no organized attempt has as yet been made to establish any system, each manufacturer having adopted whatever his judgment may have dictated as the best, or as most convenient for himself; but the importance of the works now in progress, and the extent to which manufacturing has attained, admonish us that so radical a defect should be allowed to exist no longer. The importance of this subject was long ago recognised in England, and the engineers of that country, by mutual agreement, adopted the proportions now in universal use there. Our standard of length being the same as theirs, it would seem desirable that the system which they have adopted should also be employed by us, unless grave objections can be urged against it and a better one substituted. In examining the details of their system, the first in importance appears to be the pitch or the distance from centre to centre of the threads upon each diameter of screw, which is as follows, viz:

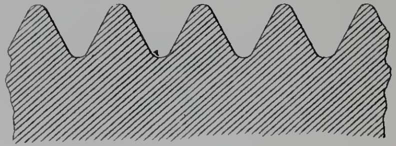
Diameter of screw, . . .	$\frac{1}{4}$	$\frac{5}{16}$	$\frac{3}{8}$	$\frac{7}{16}$	$\frac{1}{2}$	$\frac{5}{8}$	$\frac{3}{4}$	$\frac{7}{8}$	1	$1\frac{1}{8}$	$1\frac{1}{4}$
No. of threads per inch, .	20	18	16	14	12	11	10	9	8	7	7
Diameter of screw, . . .	$1\frac{3}{8}$	$1\frac{1}{2}$	$1\frac{5}{8}$	$1\frac{3}{4}$	$1\frac{7}{8}$	2	$2\frac{1}{4}$	$2\frac{1}{2}$	$2\frac{3}{4}$	3	$3\frac{1}{4}$
No. of threads per inch, .	6	6	5	5	$4\frac{1}{2}$	$4\frac{1}{2}$	4	4	$3\frac{1}{2}$	$3\frac{1}{2}$	$3\frac{1}{4}$
Diameter of screw, . . .	$3\frac{1}{2}$	$3\frac{3}{4}$	4	$4\frac{1}{4}$	$4\frac{1}{2}$	$4\frac{3}{4}$	5	$5\frac{1}{4}$	$5\frac{1}{2}$	$5\frac{3}{4}$	6
No. of threads per inch, .	$3\frac{1}{4}$	3	3	$2\frac{7}{8}$	$2\frac{7}{8}$	$2\frac{3}{4}$	$2\frac{3}{4}$	$2\frac{5}{8}$	$2\frac{5}{8}$	$2\frac{1}{2}$	$2\frac{1}{2}$

It is probable that this scale, with the exception of the half-inch, would be a fair compromise of all the proportions used in this country.

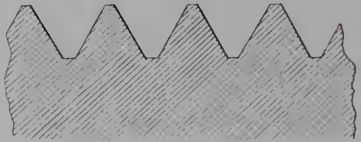
— FIG. 1 —



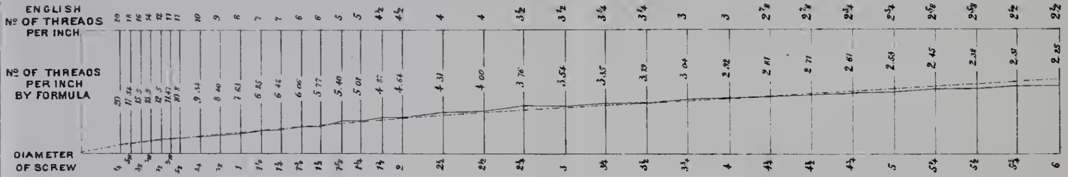
— FIG. 2 —



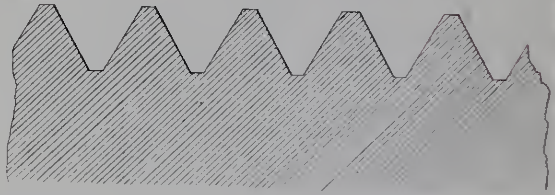
— FIG. 3 —



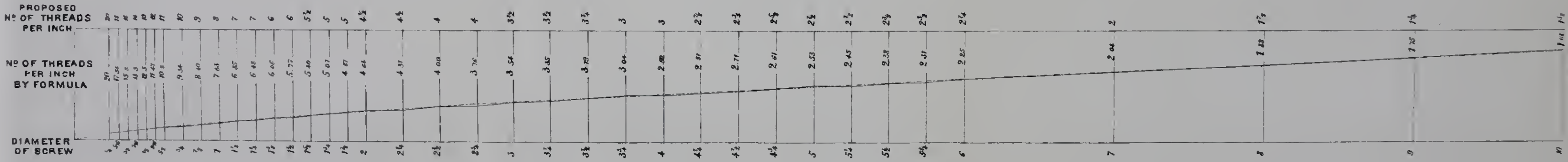
— FIG. 4 —



— FIG. 6 —



— FIG. 5 —



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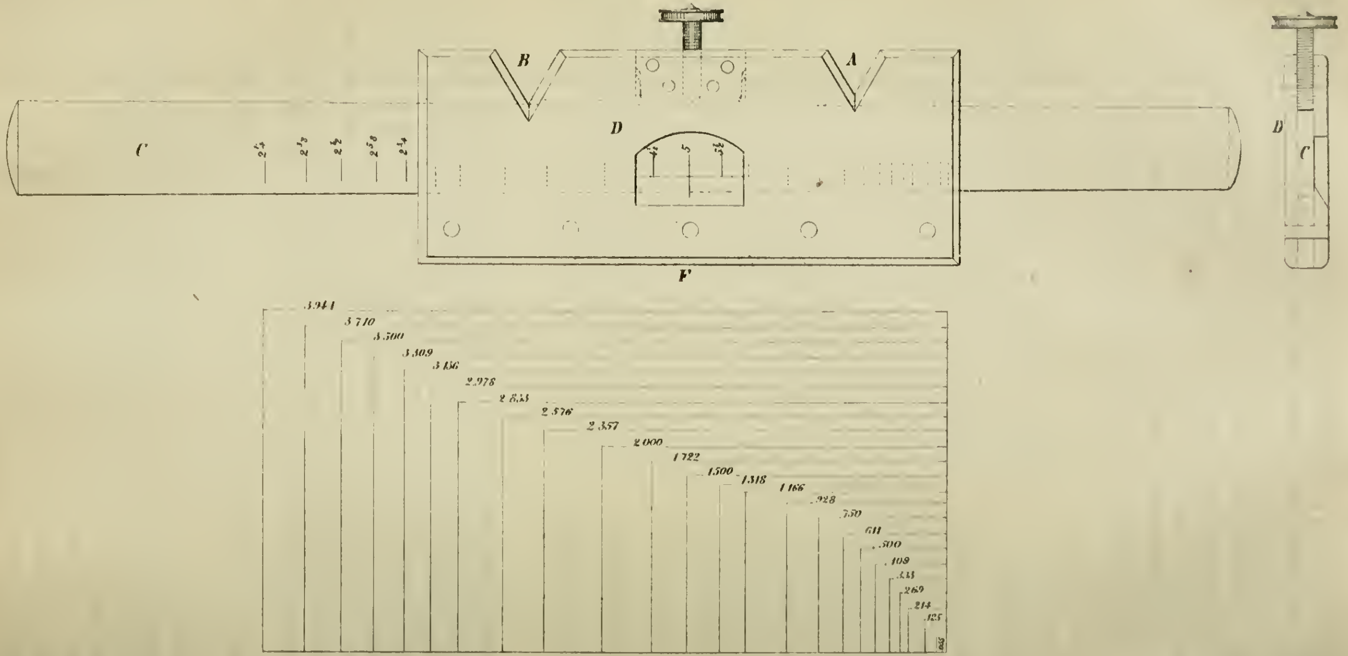

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The accompanying diagram, Plate II, Fig. 4, will serve to show the comparative relation which these pitches bear to each other and to their diameter. This indicates also that the pitch of the half-inch screw is too coarse; our practice would probably average finer. It is interesting to observe the general regularity of the curve produced by these proportions, showing, that notwithstanding they have been determined arbitrarily, they must follow some rule, and we accordingly find that the number of threads per inch may be expressed to the nearest aliquot of an inch by the formula $1 \div \frac{\sqrt{d}-a}{c}$, in which d variable = number of sixteenths of an inch in the diameter of the screw, plus ten, a = constant 2·909, and c = divisor 16·64. The table herewith, calculated to this formula, shows that the English screws, with the exception of six sizes, do not vary from the rule until they reach 4½ inches diameter, and then but slightly. It is believed the pitches as given by the formula would be found an improvement, but not sufficiently so to warrant us in adopting them, providing the other peculiarities of the English system should meet our approval.

Diameter of screw,	¼	5/16	⅜	7/16	½	⅝	¾	⅞	1	1⅛	1¼
Formula-thread, .	20	17·54	15·5	13·9	12·5	10·8	9·34	8·40	7·63	6·98	6·48
Nearest aliquot, .	20	18	16	14	13	11	10	9	8	7	7
English thread, .	20	18	16	14	12	11	10	9	8	7	7
Diameter of screw,	1⅜	1½	1⅝	1¾	1⅞	2	2¼	2½	2¾	3	3¼
Formula-thread, .	6·06	5·77	5·40	5·11	4·87	4·64	4·31	4·00	3·76	3·54	3·35
Nearest aliquot, .	6	6	5½	5	5	4½	4½	4	4	3½	3½
English thread, .	6	6	5	5	4½	4½	4	4	3½	3½	3¼
Diameter of screw,	3½	3¾	4	4¼	4½	4¾	5	5¼	5½	5¾	6
Formula-thread, .	3·19	3·04	2·92	2·81	2·71	2·61	2·53	2·45	2·38	2·31	2·25
Nearest aliquot, .	3¼	3	3	2⅞	2¾	2⅝	2½	2½	2⅜	2⅜	2¼
English thread, .	3¼	3	3	2⅞	2⅞	2¾	2¾	2⅝	2⅝	2½	2½

The form of thread adopted by the English Engineers is one, with flat sides at an angle to each other of 55°, with a rounded top and bottom. The proportions for the rounded top and bottom are obtained by dividing the depth of a sharp thread having sides at an angle of 55°, into six equal parts and within the lines formed by the sides of the thread and the top and bottom dividing lines, inscribing a circle which determines the form of top and bottom of thread, as illustrated by Fig. 2, Plate II. Judging from the practice of this country, the

English form of thread has not met with the same favor that has been accorded to their pitches. Its advantages over the sharp thread are, increased strength to the screw from the absence of acute corners, and the greater security from accidental injury which the rounded top possesses. Its objectionable features are, first, that the angle of 55° is a difficult one to verify; it is probable, no gauges to this angle, made independently of each other and without special tools, would correspond with sufficient accuracy. Secondly, the curve at the top and bottom of the thread of the screw will not fit the corresponding curve in the nut, and the wearing surface on the thread will be thus reduced to the straight sides merely. It is not to be inferred from this that these curves cannot be made to fit, but only that the difficulties in producing contact are so much increased by the peculiar form, that in practice it will not be accomplished. Thirdly, the increased cost and complication of cutting tools required to form this kind of thread in a lathe, it being requisite that this tool shall have at least three cutting sides, in order to form the round top between two of them. The English practice for small work, is to rough out in a slide-lathe with a single-point tool having sides of the proper angle, and finish in a hand lathe with a comb chaser, which has been dressed to the proper form upon a hob kept for that purpose, requiring three kinds of cutters and two lathes to perform what with our practice requires but one cutter and one lathe. On large work, the screw is finished in the slide lathe, with a chasing tool dressed to the proper form upon a hob; and as these hobs are necessarily the standards of form until worn out, it is fair to suppose the shape must be undergoing a continual change. The necessity of guarding the edge of the thread from accidental injury, becomes more and more apparent as the size of the bolt is increased, and we have recognised this by finishing such bolts with a small flat upon the top of the thread; but no plan has been proposed for general adoption upon all screws, nor have any proportions been suggested where a flat is desired, or where from the size of the bolt it would seem to be necessary. As it is very desirable that some uniform rule should be observed in the formation of all threads, and as the sharp top is objectionable upon large screws, this form must be abandoned if we would accomplish our object. It being conceded that the flat angular sides are necessary, we have only to choose between the rounded and flat top; and having examined the former, it only remains to notice whether the flat will be found free from the objections urged against the round. As the sides of the thread are the only parts requiring to be fitted, and as these are of the same shape as the sharp thread, the one will be as easily made as the other. The width of the flat top will be determined by the depth to which the thread is cut, so that the same tool can be used in both cases. The flat on the top of the thread being required to protect it from injury, it is evident a similar shape at the bottom would give increased strength to the bolt as well as improve its appearance. To give this form requires only that the point of the cutting tool shall be taken off and then it is evident this thread can be cut in a lathe with the same tool and in the same manner as the sharp thread. The width of flat in the bottom of thread being dependent upon the amount taken off the point of the tool, it becomes necessary not only to determine what this amount shall be, but also to provide a means of measuring it. The proportions for the proposed thread and its comparative relation to the sharp and rounded threads, will be readily understood from the accompanying diagrams, Plate II, in which Fig. 1 represents a sharp thread, Fig. 2 a rounded top and bottom to the English proportions, and Fig. 3 the flat top and bottom, all of the same pitch. The angle of the proposed thread is fixed at 60°, the same as the sharp thread, it being more readily obtained than 55°, and more in accordance with the general practice in this country. Divide the pitch, or, which is the same thing, the side of the thread, into eight equal parts, take off one part from the top and fill in one part in the bottom of the thread, then the flat top and bottom will equal one-eighth of the pitch, the wearing surface will be three-quarters of the pitch, and the diameter of screw at bottom of the thread will be expressed by the formula $\text{diam.} - \frac{1{\cdot}299}{\text{per in.}}$. These proportions will give the depth of the thread almost precisely the same as the English, and as the wearing surface on all screws will be confined practically to the flat sides, we shall find that upon the proposed plan this will be 36 per cent. greater than on the English. The accompanying drawing, Plate III, represents a gauge, designed for measuring the flat upon the chasing tool. D is the main frame of the instrument made in three thicknesses, so that the central pieces may be hardened and ground to shape, the outer pieces serving to keep the central ones in their proper positions. A is an angle of 60° cut in the edge of the frame D, and having its two sides to form equal angles with the edge of the frame. B is similar to A, but having the apex of the angle cut away so as to permit the wedge piece C to pass above the point of junction of the two sides of B. The side of C is graduated, so that upon bringing any particular mark to coincide with a fixed point or mark in the frame D, the edge of C will pass the proper distance above the apex of the angle B, which will indicate the amount to be taken off the point of the chasing tool which has first been accurately fitted to the angle A. In setting the chasing tool in the lathe, the side F of the gauge should be placed against the surface of the work to be cut, which is suspended upon the centres, and while in this position the chasing tool must be adjusted to it and securely fastened. This will insure the thread when cut being perpendicular to the surface. The width of the flat top and bottom may also be obtained by the use of a chasing tool having a flat on its end of known width, but less than required in the bottom of the thread, the depth of the thread having been determined, the tool may cut in to this point and finish by a side movement sufficient to give the required amount of flat.

A system of uniform dimensions for bolt-heads and nuts being intimately connected with the subject just discussed, it is believed that a convenient formula that would express the required size would go far towards inducing a uniform practice and with this view the following formulæ and tables for screw threads and nuts are offered for the acceptance of our Engineers. Should they meet the approval and be adopted by any considerable portion of the profession, there is every reason to believe they would soon be applied universally, and to enable a comparison to be readily instituted between the systems which have been discussed, diagrams Figs. 5 and 6, Plate II, have been prepared, the former representing the pitch as obtained by the formula, with that proposed to be adopted, and the latter representing a section of a ten inch bolt having its threads to the same proportion. To compare the form of thread, two of the $\frac{7}{8}$ and 2 in taps exhibited are by Whitworth & Co., to the English standard and the other two of same size are to the proposed thread. Formulæ:

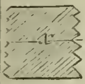
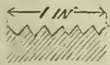
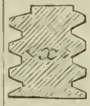
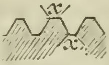
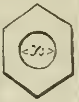
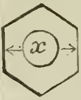
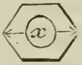
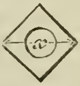
Δ = diameter of screws.
d = No. of sixteenths plus ten in Δ.
x = pitch of screws.
v = No. of threads per in.
a = constant 2·909.
c = divisor 16·64.
m = flat top and bottom.
n = diameter of screws at bottom.
s = short diam. of nut or bolt-head = $1\frac{1}{2}$ diam. of screws + $\frac{1}{8}$ in.
u = long diam. hexagonal nut or bolt-head.
t = long diam. square nut or bolt-head.

$$x = \frac{\sqrt{d} - a}{c} \qquad v = \frac{1}{x} \qquad t = s \times 1{\cdot}414$$

$$m = \frac{x}{8} \qquad n = \Delta - \frac{1{\cdot}299}{v}$$

$$s = \frac{3\Delta}{2} + \tfrac{1}{8}\text{-in.} \qquad u = s \times 1{\cdot}155$$

PROPORTIONS OF BOLTS AND NUTS.

Diameter of bolt.	Number of threads per inch.	Diameter of root of thread.	Width of flat top, bottom of thread.	Size hole in nut.	Short diameter of hex-nut.	Long diameter of hex-nut.	Long diameter of sq. nut.
							
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5/16	18	·240	·0070	·240	19/32	11/16	13/16
3/8	16	·294	·0078	·294	11/16	25/32	31/32
7/16	14	·344	·0089	·344	25/32	43/48	1 1/16
1/2	13	·400	·0096	·400	7/8	1	1 1/4
9/16	12	·454	·0104	·454	31/32	1 7/64	1 5/16
5/8	11	·507	·0113	·507	1 1/16	1 7/32	1 1/2
3/4	10	·620	·0125	·620	1 1/4	1 7/16	1 3/4
7/8	9	·731	·0140	·731	1 7/16	1 21/32	2 1/32
1	8	·837	·0156	·837	1 5/8	1 7/8	2 5/16
1 1/8	7	·940	·0180	·940	1 13/16	2 3/32	2 1/2
1 1/4	7	1·065	·0180	1·065	2	2 5/16	2 27/32
1 3/8	6	1·160	·0210	1·160	2 3/16	2 1/2	3 1/10
1 1/2	6	1·284	·0210	1·284	2 3/8	2 3/4	3 3/8
1 5/8	5 1/2	1·389	·0227	1·389	2 9/16	2 15/16	3 5/8
1 3/4	5	1·490	·0250	1·490	2 3/4	3 3/16	3 29/32
1 7/8	5	1·615	·0250	1·615	2 15/16	3 13/32	4 3/16
2	4 1/2	1·712	·0280	1·712	3 1/8	3 5/8	4 7/16
2 1/4	4 1/2	1·962	·0280	1·962	3 1/2	4 1/16	4 31/32
2 1/2	4	2·175	·0310	2·175	3 7/8	4 1/2	5 1/2
2 3/4	4	2·425	·0310	2·425	4 1/4	4 29/32	6
3	3 1/2	2·628	·0357	2·628	4 5/8	5 3/8	6 9/16
3 1/4	3 1/2	2·878	·0357	2·878	5	5 3/4	7 1/8
3 1/2	3 1/4	3·100	·0384	3·100	5 3/8	6 7/64	7 5/8
3 3/4	3	3·317	·0410	3·317	5 3/4	6 5/8	8 3/16
4	3	3·566	·0410	3·566	6 1/8	7 3/64	8 11/16
4 1/4	2 7/8	3·798	·0435	3·825	6 1/2	7 1/2	9 1/4
4 1/2	2 3/4	4·027	·0460	4·027	6 7/8	7 31/32	9 3/4
4 3/4	2 5/8	4·255	·0480	4·255	7 1/4	8 3/8	10 9/32
5	2 1/2	4·480	·0500	4·480	7 5/8	8 13/16	10 13/16
5 1/4	2 1/2	4·730	·0500	4·730	8	9 1/4	11 3/8
5 1/2	2 3/8	4·953	·0526	5·053	8 3/8	9 11/16	11 29/32
5 3/4	2 3/8	5·203	·0526	5·203	8 3/4	10 1/8	12 7/16
6	2 1/4	5·423	·0555	5·423	9 1/8	10 9/16	12 9/10

Messrs. Merrick & Sons submitted for examination specimens of a series of screw threads, bolts, and nuts, from 6 inches to ⅜ths of an inch, as used by them. The pitches of these screws are almost indentical with those proposed in the paper read by Mr. Sellers.

Messrs. Bement & Dougherty submitted drawings of bolts and nuts as used at their establishment (the Industrial Works), with explanatory remarks showing that their pitches of threads were the same as Whitworth's except the half-inch, which is finer (13); and the heads and nuts are invariably hexagonal; the diameter of parallel sides of finished heads and nuts being one-and-a-half times the diameter of the bolts. The thickness of nut equals the diameter of bolt, and the thickness of bolt-head ⅞ths of the diameter of bolts.

Mr. J. A. Millholland, Master Mechanic and Assistant Superintendent of the Philadelphia and Reading Railroad Company, submitted the following proportions for threads of bolts, and size of bolt-heads as used by that Company.

Reading Railroad Repair Shop, Reading Pennsylvania.

Size of "Tap."	Threads to the Inch.	Size of Bolt-heads.
½-inch.	12	⅞
⅝ "	12	1⅛
¾ "	10	1¼
⅞ "	9	1½
1 "	9	1⅜
1⅛ "	8	1¾
1¼ "	8	1⅞
1⅜ "	7	2
1½ "	7	2¼

Mr. C. T. Parry expressed a hope that something practical and useful might result from the introduction of the subject of screw threads, beyond the mere reading of the paper, and publishing it in the *Journal*. He said that the want of uniform standard threads has long been felt, and suggested that copies of papers might be sent to the principal engineers of the country, and their opinion solicited with a view of arriving at something acceptable to all parties. He was in favor of finer threads than that used by the English, or the thread proposed in the paper read by Mr. Sellers, particularly for locomotive engine work; but was willing to make concession of opinion for the sake of uniformity. M. W. Baldwin & Co., locomotive builders of this city, with which works he is connected, use on

¼-in. bolts,	28 threads,	¾-in.	10 threads,	1¼-in.	8 threads,
⅜ " "	22 "	⅞ "	10 "	1⅜ "	8 "
½ " "	16 "	1 "	10 "	1½ "	8 "
⅝ " "	14 "	1⅛ "	10 "		

on all permanent bolts, 1¼ inch diameter, they use 10 threads. They consider 10 threads on ¾ bolts coarse, but as they are used principally for stuffing-box glands, and require movable nuts which can be readily run off by the hand, they have adopted this size. They are of the decided opinion, from long experience, that the fine threads are the best adapted for locomotive purposes, as they are more permanently secured when screwed up, than the coarse threads. The threads used by them are abundantly strong to tear asunder the bolt before stripping, and the bolts are much stronger than if coarse threads were used. He was of the opinion that on locomotives especially, all known precautions should be used against accidents that might result from losing bolts, as the machinery, for the most part, is out of sight of the engineer, whilst running over rough roads, which is not the case in stationary machinery; and for this reason they use finer threads, double or jam nuts and through-keys in all bolts, from which by coming loose, accidents might occur. We would be glad to find a coarser thread acceptable, and would be willing to advocate it if by that means only, uniform standard threads could be adopted.

On motion of Mr. J. H. Towne, seconded by Mr. Washington Jones, it was

Resolved, That the Corresponding Secretary of this Institute, be instructed to send a copy of the paper read this evening, on screw threads and nuts, to the American Institute, New York, and to other similar societies, with the request that they will give it an early consideration with a view to promote the introduction into general use of the system advocated, if they shall approve of the same.

On motion of Mr. Algernon Roberts, seconded by Mr. C. T. Parry, it was

Resolved, That the following gentlemen be a committee to investigate the question of a proper system of screw threads, bolt-heads, and nuts, to be recommended by this Institute, for the general adoption by American engineers.

Committee.—W. B. Bement, C. T. Parry, J. V. Merrick, J. H. Towne, C. Sellers, B. H. Bartol, Edw. Longstreth, Jas. Moore, and J. W. Murphy.

Mr. E. Longstreth exhibited an instrument for setting the tools of screw-cutting lathes, so as to bring the point of the tool at right angles with the surface to be cut. It consists of a piece of steel one-and-a-half inches long, with a groove front which is laid against the bar; on the back, and at right angles with the groove, is a notch to receive the tool point.

Mr. L., also exhibited an instrument for testing the correctness of screw threads.

Mr. Chase read the following conclusion of the abstracts of his papers on "Tides of Rotation."

On account of the mutual dependence of all the forces of nature, and the reasonableness of Prof. Faraday's conjecture, that they are often, if not always, convertible more or less into each other,* it seems probable that the disturbances of the magnetic needle may be as closely connected with the earth's rotation, and the continually changing position of each point relatively to the sun, as those of the barometer and thermometer. Ampère held that the earth is an electro-magnet, magnetized by an electric current from east to west, the current being excited by the action of the sun's heat successively on different parts of the earth's surface as it revolves toward the east. The friction of trade-winds and ocean-currents and the variations of light and temperature that are produced by rotation and orbital revolution, must exert an influence upon the magnetic needle, and besides these indirect effects, M. Arago showed that simple rotation, in some unknown way, produces magnetism in bodies of every description. Many have supposed that this magnetism is derived from the earth by induction, but on account of the impossibility of escaping from the influence of terrestrial magnetism, it is difficult to obtain any conclusive evidence on the subject.† A similar impossibility has interfered with Prof. Faraday's endeavors to connect gravity and magnetic or electric action by experimental results. The probability of such a connexion has been shown by the electricity developed in the dry pile of De Luc, and by Gen. Sabine's observation, that when the sun and moon were on the meridian, the magnetic variation reached 5°, but when they were in quadrature, it fell as low as 20′.‡

The great forces of nature can be measured only by their disturbances or their deviations from uniformity. The action of gravity is so nearly uniform at all times and in all parts of the globe, that it is difficult to imagine any crucial experiment that could demonstrate its relations to magnetism. Perhaps a needle, hinged at its point of support, with the two extremities nicely balanced, might help us towards such a demonstration, if careful experiments were tried, to show the relative influence of gravity upon each extremity, both before and after magnetizing, and when subjected to artificial magnetism, so as to produce various amounts of deviation from the normal dip and declination. Or, centrifugal force, so applied as alternately to assist and oppose the effects of gravity, as in large fly-wheels revolving with various degrees of rapidity, may indicate variations of magnetic influence, that can be explained only by the conversion of gravity into magnetism or the reverse.

Prof. Faraday, in a lecture before the Royal Institution in the year 1857, endeavored to show that our usual conception of gravity is not in harmony with the principle of "conservation of force." Prof. Brücke § and others, have tried to remove the difficulties in which the question is involved, but I believe none of the proposed solutions have been satisfactory to the learned philosopher who first started the discussion.

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* Phil. Mag., Fourth Series, 1, 68.

† See correspondence of M. J. Nickles, Silliman's Journal, v. xvii. p. 117, &c.

‡ Silliman's Journal, v. xix p. 424.

§ Phil. Mag., Fourth Series, 15, 81.

It has even been questioned whether gravity can be properly called a force, or whether it is anything more than a simple "tendency." Prof. Brücke has shown conclusively, that it is subject, like heat and other recognised forces, to all the laws which regulate the interchange of *actual* and *potential* energy, and our barometrical investigations furnish a beautiful illustration of the manner in which its tension is balanced by opposing forces.

We speak, indeed, of weight, as if it could be predicated only of bodies at rest, and as if it were so entirely distinct from momentum that no comparison could be properly instituted between the two. Precisely the reverse is true. Absolute rest is apparently an impossible condition of matter, for, to whatever extent the action of opposing forces may be relatively neutralized, the inconceivable rapidity of æthereal, planetary, and stellar motions, produces a constant change of place. Even if we confine our attention to the earth alone, in each instant (dt), every particle has a tangential motion (tan. $d\theta$), and a central motion of gravity (sin. $d^2\theta$) that constitutes a *vis viva* which we call its weight, and which is in equilibrium with the elasticity of the molecular æther. The sum of all the instantaneous energies is the same, whether the particle fall freely for any given time, or remain apparently at rest. All the potential energy which is transformed in one case into the actual energy of motion,[*] in the other is counteracted by an equivalent and opposite central energy of elasticity. Therefore, when we compare the relative effects of rotation and gravity, it is immaterial whether we use as the measure of force, the integral of the *vires vivae*, or the respective amounts of motion that the two forces would produce, if they were able to act freely for the same time. The difficulty of determining the repulsion of molecular elasticity precluding any satisfactory use of the former measure, I employed the other, and the precise accordance of the results thus obtained, with the results of observation, justified the correctness of the hypothesis, in the same manner as the accurate computation of planetary motions has confirmed the Newtonian theory of gravity.

Gravity, therefore, with the same propriety as heat, may be considered as a "mode of motion," whether acting merely as "dead weight," or as an accelerating or a deflecting force. If it can be shown that magnetism also originates in motion, we may be able to demonstrate the mutual convertibility that Faraday suspected.

The earliest hypothesis with regard to terrestrial magnetism looked for its cause to a powerful magnet, lying nearly in the line of the earth's axis. Subsequent discoveries led to a modification of this view by the supposition of another magnet, pointing towards the Siberian pole. Mr. Barlow's idea, that the magnetism is superficial and induced,[*] has now been generally adopted, and if it could be shown that solar or rotary action is capable of developing magnetism in particles such as those which are known to constitute our globe, the great difficulty in the way of satisfactory explanation would be removed.[†] Ampère's, Barlow's, and Christie's experiments showed that simple rotation is sufficient to affect the magnetism of a compass-needle,[‡] and in the oxygen of the atmosphere, which, as Faraday discovered, has a specific magnetism, variously estimated at from $\frac{1}{882}$[§] to $\frac{1}{287}$[||] of that of iron, we have a medium through which any induced magnetism may be distributed over the entire surface of the earth. Some simple experiments that can be easily repeated, seem to confirm Ampère's views, and to indicate the manner in which the circulating electric current is excited.

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[*] The potential energy of gravity is represented by $g = 32$ ft. per second. The earth's rotation allowing only about $\frac{1}{290}$ of this amount, or ·1107 ft. per second, to be converted into actual energy, the remainder must be employed in overcoming molecular elasticity. The formula $a = \left(\frac{gt^2 r^2}{4\pi^4}\right)^{\frac{1}{3}}$ gives 26,221 miles as the radius of the sphere of attraction that is in equilibrium with the molecular elasticity at the earth's surface. These opposing forces must produce constant oscillations, and by the study of these oscillations, it may perhaps be possible to reconcile the several hypotheses of Newton, Faraday, Mossotti, and Challis, respecting the nature of gravitation. See Phil. Mag., Fourth Series, v. xiii. p 231-7, and v. xviii. p. 447, sqq.

There is a species of mechanical polarity, of which I have never seen any notice, that is apparently produced by motions, resembling those to which the air is continually subjected. It may be exhibited in the following ways:

1. In the middle of a basin of water, lay a long strip of any substance (floating it by corks or otherwise, if it is heavier than water). After the water has become still, lift the basin carefully by one hand, and hold it at arm's length. The intermitting muscular action produces longitudinal vibrations, which tend to bring the floating strip into a line with the outstretched arm, and the tendency may be increased by moving the basin gently up and down.

2. Hold the gimbals of a binnacle compass so that it can swing only in one direction, and cause it to move like a pendulum in that direction. The needle will tend towards the line of oscillation. Vessels may have been lost from ignorance of this fact, for it is not unusual for compass pivots to become so worn that the needle moves sluggishly, and in order to start it, the compass box is shaken. If one of the gimbal hinges should be rusty, the shaking would bring the needle into a line perpendicular to the axis of the free gimbal, and the captain might easily suppose that he was sailing north, when his course was due east or west.

3. Take an ordinary pocket compass, grasp it firmly between the thumb and finger of one hand, and move it quickly up and down through a small arc. The needle, as in the last instance, will tend towards the plane of motion. This experiment may be variously modified, according to the length and directive energy of the needle, the steadiness of the operator's nerves, &c. Sometimes a simple grasp, with a powerful

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[*] Phil Trans., 1831.

[†] Enc. Brit., Art. "Magnetism."

[‡] The effect of rotation on the magnetic needle may be shown in a rough way, by causing an ordinary grindstone to revolve rapidly, and bringing a compass near its edge.

[§] By M. Becquerel.

[||] M. Plücker.

muscular contraction, will bring the needle into line, without any other vibration than that which arises from the irresistible nervous tremor. Sometimes the momentum acquired by each pole in its approach to the operator, carries it forward so as to bring the other pole under the wave-influence, and the needle is thus made to rotate so rapidly as to become nearly invisible.

The polarity in each of the three cases here enumerated, is easily explained upon purely mechanical principles, but there are some indications that seem to show a close connexion between the mechanical vibrations and those of nervous electricity. There appears to be a great difference in the control of different individuals over the needle. Some can bring it into line at once, with scarcely any perceptible motion, while others are obliged to use considerable effort; the needle does not seem at all times equally susceptible; it often appears more easy to produce rotation in one direction, than in the other. There may, therefore, be a natural connexion between these experiments and those of M. Du Bois Raymond, who attached two strips of platinum to a very delicate galvanometer, and caused them to dip into two cups of salt water. Dipping the fingers of each hand into the cups, and alternately bracing the muscles of each arm, he produced a perceptible deflection of the needle. MM. Becquerel and Despretz repeated the experiment without obtaining very satisfactory results, but M. Humboldt was more successful.* Add to these phenomena the well-known evidences of a constant current, circulating around magnets, and if we suppose that electricity consists simply of vibrations, it will seem perfectly natural that the magnet should obey the strongest vibrations.

The combined effect of solar action, trade-winds, and prevailing aerial currents, in producing mechanical polarized vibrations that must influence the magnetic needle, was illustrated by diagrams.

Mr. John W. Nystrom, Civ. Eng. made the following remarks:—On Metrology and Arithmetic.

As this is a subject which has occupied my attention for many years, and cost me much time and money, I was very glad to hear at the meeting in February, that the Franklin Institute appointed a Committee on Weights, Measures, and Coins, and that the honor was conferred on me, of being one of its members.

It was my desire to take an active part in the proceedings of that Committee, but early in March, business called me to the Northern States and I had only the pleasure to attend the first two meetings, which afforded me the greatest interest, not that I expected we would come to any satisfactory conclusion on metrology, or that we could accomplish in one month what has puzzled our ancesters for centuries.

The International Decimal Association has, for the last ten years, labored very hard on that subject, and, as yet, accomplished comparatively nothing; and if proper precaution be not taken, our result will likely be the same.

It is not reasonable to suppose that only a few members represent-

* See Silliman's Journal, v. viii.

ing only one branch of the Arts, (as was the case in the last meeting I had the pleasure to attend,) would be able to accomplish an object of such paramount magnitude and destination.

The object of our Committee ought to be, to devise a system of metrology that would in all its bearings fulfil all the requirements of the nation, or of mankind. In the accomplishment of this, members must necessarily represent all the different Arts and Sciences; as Astronomy Navigation, Geography, Geology, Chemistry, Pharmacy; and in the Arts, the different branches of the Mechanics, as Shipbuilders, Marine-Engine builders, Locomotive builders, Tool-makers, Bridge builders, Surveyors, Architects, &c. And in the Trade, ought to be represented the Market, Custom-House, Bankers, &c., &c.

In case we have not members of the Institute to represent all these different Arts and Sciences, I would propose to invite "outside representatives."

The Committee should consist of at least twenty or thirty members, and constitute a standing committee of metrology until the object is accomplished.

Philadelphia (a City of nearly a million inhabitants,) ought to be able to furnish the requisite materials for this subject, so highly important to our nation, and it is our most solemn duty to give the subject our serious attention, in hope of removing one of the heaviest burdens ever instituted on mankind.

This is not easily accomplished, and should not be hurried. We know what inconveniences it causes, and how little we are disposed to change old habits, and when we shall come to see and recognise the necessity of a change in metrology, we ought to take care not to expose our generation to an unremunerative inconvenience, to be abolished by their descendants.

In the two meetings on Weights and Measures, which I had the pleasure to attend, the Committee appeared to incline to the decimal system, and I believe it was proposed to decimate our old units. The French metrical system was also spoken of, but did not seem to gain much favor.

I have, on former occasions, alluded to the imperfection and inconveniences attending the decimal system, and proposed as a substitute, a binary system of arithmetic and metrology, fully described in the Journal of the Franklin Institute.

The subject is of such great importance that I do not hesitate to allude to it again. We know our ancestors have been laboring on this great problem for centuries, and it is my own conviction that we are not better off to-day in metrology, than they were in the middle ages; for the little benefit derived from the decimal system in book-keeping does not compensate the inconveniences it causes by depriving the people of their natural conception in counting.

As long as we confine ourselves to decimals, we have nature against us, or rather, we are struggling against nature.

I am at present engaged in working out a new system of constructing ships, where I am at liberty to arrange it according to any system I please; but the decimal system is the very last I would adopt. I have now arranged it into a binary or octonal system, and that, mind, when I am obliged to employ decimal arithmetic. In working out the tables I have over one hundred thousand multiplications to make, each of five figures by seven. I cannot beneficially employ logarithms, because it is the logarithms themselves to be multiplied, and they do not extend to the requisite number of figures.

A binary arithmetic would reduce that immense labor to only about one-fifth. The case is the same with all decimal calculations. Will you then wonder that I advocate a binary arithmetic?

I am well convinced that life and property are constantly wasted; our comfort and progress considerbly retarded, for the want of a natural system of counting.

The decimal arithmetic must be pumped into us, by the routine of schooling, and those who have not the opportunity of such education, are left to the mercy of consequences, while a binary system would come natural to us like music.

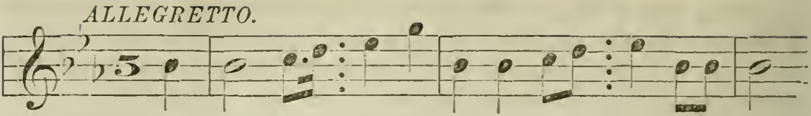
Our mental conception of decimal arithmetic is restricted to a very narrow limit,—most simple examples must be set down on paper, and even then may be troublesome in the solution; while a binary arithmetic would, in most cases, dispense with the mechanical operations in calculations; difficult examples of which we have now no conception, could be managed mentally like that of a musician who plays a difficult piece of music by the ear.

There is a very striking resemblance between arithmetic and music, the latter we know is arranged by nature in a perfect binary system, which can be managed by a child before it can read or even before it can count to 10. If music was arranged in decimals, it would be as difficult to learn as our present arithmetic, and we could not catch up a song by the ear.

We have some very faint indications of attempts having been made to arrange music into decimals. Mr. Wm. H. Fry, who lectured on music in this city some years ago, said, "Five-beat time is scarcely ever used, and then only for crazy chorus, or to produce pandemonic effects," and such is the case with the decimal arithmetic; it produces a pandemonic effect on mankind.

I know of only one case on record of decimal music, namely the tenor solo in the second act of the opera "La Dame Blanche," composed by Boieldieu, late Professor of Composition in the Concervatoire of Music, Paris. Professor Boieldieu was a Scientific man. He evidently attempted to try the decimal system of music, and succeeded only to arrange a five-beat bar,—but the subdivision of the beats, as well as of the burdens, are still binary as required by nature.

I beg to detain you a few minutes more, by illustrating this music on the black-board, to explain how unnatural the decimal system is to the mind.



This is the five-beat bar *invented* by Prof. Boieldieu. When it was first produced in Paris, some 40 years ago, the musicians in the audience, in which Doctor Meignon now of this city, was present, failed to make out what it was. When the musicians first tried it in rehearsal, they broke down in the middle of it, with no little amusement, and it was found necessary to sub-divide the bars into two parts, making three beats it the first, and two in the second as shown by the dots, which is the form in which that music now appears in the market. This shows how unnatural it is to the mind to divide by five.

I cannot bring to my recollection any rule in geometry by which to divide a straight line into five or ten equal parts, except by the aid of projection; and if there is such a rule it cannot be simple, but complicated like the decimal system.

Music is employed to lubricate our movements and feelings, like oil in machinery. We use music to dance by, to march by; and we use music in hauling and hoisting. Should decimal music be started in a ball-room, the dancer would find himself obliged to lead the bars sometimes with his right and sometimes with his left foot, and could not possibly come right at the end of the burden, which would surely cause confusion. If decimal music should be drummed for a marching regiment, the soldiers would surely step on one another's heels. I would not trust to such music in hoisting a boiler on board a steamboat, for it would operate the same as if a horse would attempt to take three steps with his fore feet, while two with his hind feet; and in like manner does the decimal system operate in almost every transaction in our retail dealings.

The other day I went to Germantown, bought two tickets at the railway station, the price of each was

	Decimal.	Binary.
	12 cents	2
Paid with	50 "	8
Received in change	25+1 "	4

which is a very common, but odd transaction.

Let us now see what it would be with a binary system. The dollar divided into halves and halves, say into 16 parts, when 50 cents would be represented by 8 which is half of 16, the price by 2, and the change by 4. Here you will find that the binary system not only gives the simplest and smallest numbers for our mental conception, but we would have coins to suit it exactly.

I have here a bill of amusement which says, "Panorama of the Bible."

	Decimal.	Binary.
Commences at	7½ P.M.	5
Matinees,	3	2
Admittance,	25 cents.	4
Children half-price,	15 cents.	2

you see it is all odd and wrong for children.

A binary system will give an even hour for 7½ o'clock, because 7½ : 24 = 5 : 16.

The binary column you see contains the most simple figures, with the desired price for children.

I shall not detain you any longer by pointing out cases of these endless inconveniences, which we are so accustomed to, that we seldom notice them, but by some reflections we find it in every transaction. The even and easy numbers for counting, such as 2, 4, 6, 8, 12, 16, 24, 32, &c., must, by the decimal system, be piled up by odd coins and measures.

These inconveniences are more conspicuously noticed by me, on account of having paid attention to the subject for a great many years, and practiced a binary system of arithmetic.

In chemistry we know that the elementary substances combine in binary proportions.

The binary theory in chemical compounds is a very recent discovery. If we had had a binary arithmetic, it is very likely that that important discovery would have been made long before. Another discovery of equal, or perhaps of greater importance, yet to be made, is the physical constitution of heat. The road to that discovery is indicated by the numbers representing the different properties of matter, as specific and latent heat, smelting and boiling point, conducting power for heat, specific gravity, atomic weight, cohesive strength, &c., &c., all of which appear to have some binary relation to one another, but their decimal numbers only present a heap of incongruous figures to the mind.

It shows, however, how weak we are, when we unnecessarily allow ourselves to drag this heavy burden along with us ; and the more that it is propagated and advertized in most of the scientific journals of the day. European journals often devote their pages and columns to the decimal system, unconsciously extolling and expatiating on its merit.

I am not afraid, or do not hesitate to advocate a binary system of arithmetic and metrology.—I know I have nature on my side,—if I do not succeed to impress upon you its utility and great importance to mankind, it will reflect that much less credit upon our generation, upon our scientific men and philosophers.

A Comparison of some of the Meteorological Phenomena of APRIL, 1864, *with those of* APRIL, 1863, *and of the same month for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	April, 1864.	April, 1863.	April. for 13 years.
Thermometer—Highest—degree, .	75·00°	71·00°	88·00°
“ “ date, .	24th.	12th.	24th, 1861.
“ Warmest day—Mean,	68·17	60·17	74·30
“ “ “ date,	24th.	12th.	29th, 1856.
“ Lowest—degree, .	32·00	30·00	20·00
“ “ date, .	5th.	4th.	7th, 1857.
“ Coldest day—Mean,	37·17	33·33	27·70
“ “ “ date, .	5th.	4th.	2d, 1857.
“ Mean daily oscillation,	15·40	14·90	16·62
“ “ “ range, .	5·27	5·41	6·24
“ Means at 7 A.M., .	44·90	43·95	45·45
“ “ 2 P. M., .	55·58	54·35	57·12
“ “ 9 P. M., .	48·45	47·40	49·21
“ “ for the month,	49·64	48·57	50·59
Barometer—Highest—Inches, .	30·048 in.	30·185 in.	30·518 in.
“ “ date, . .	22d.	21st.	3d, 1854.
“ Greatest mean daily press.	30·016	30·177	30·458
“ “ date, . .	22d.	21st.	3d, 1854.
“ Lowest—Inches, .	29·310	29·260	28·884
“ “ date, . .	2d.	2d.	21st, 1852.
“ Least mean daily press.,	29·377	29·387	28·959
“ “ date, . .	2d.	2d.	21st, 1852.
“ Mean daily range, .	0·122	0·164	0·168
“ Means at 7 A. M., .	29·730	29·788	29·814
“ “ 2 P. M., .	29·699	29·752	29·773
“ “ 9 P. M., .	29·755	29·808	29·804
“ “ for the month, .	29·728	29·782	29·797
Force of Vapor—Greatest—Inches,	0·495 in.	0·404 in.	0·611 in.
“ “ “ date, .	25th.	30th.	22d, 1853.
“ “ Least—Inches, .	·096	·072	·066
“ “ “ date, .	7th.	9th.	13th, 1852.
“ “ Means at 7 A. M.,	·219	·207	·230
“ “ “ 2 P. M.,	·232	·213	·243
“ “ “ 9 P. M.,	·236	·232	·248
“ “ “ for the month,	·229	·217	·240
Relative Humidity—Greatest—per ct.	92 per ct.	96 per ct.	100 per ct.
“ “ “ date,	10th.	24th.	Often.
“ “ Least—per ct.,	18·0	15·0	13·0
“ “ “ date, .	7th.	26th.	13th, 1852.
“ “ Means at 7 A.M.,	71·0	68·8	71·5
“ “ “ 2 P.M.,	52·2	50·4	51·3
“ “ “ 9 P.M.,	67·8	68·2	67·6
“ “ “ for the month	63·7	62·5	63·4
Clouds—Number of clear days,* .	4	8	8·5
“ “ cloudy days, .	26	22	21·5
“ Means of sky cov'd at 7 A.M.,	62·7 per ct.	57·0 per ct.	62·8 per ct.
“ “ “ “ 2 P.M.,	73·3	75·3	66·6
“ “ “ “ 9 P.M.,	69·0	58·0	53·7
“ “ “ for the month,	68·3	63·4	61·0
Rain and melted Snow—Amount, .	4·478 in.	7·294 in.	4·966 in.
No. of days on which Rain or Snow fell,	14	16	13·2
Prevailing Winds—Times in 1000,	N 13°23′ W ·133	N 30°8′ W ·223	N 61°21′ W ·153

* Less than one-third covered at the hours of observation.

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CIVIL ENGINEERING.

For the Journal of the Franklin Institute.

The Arch Truss Girder again.—More upon the New System.

By S. W. Robinson, C.E., Detroit, Michigan.

My article in the March Number of your Journal, contained in its latter portion the statement, *without the proof*—of a few principles and formulæ as applied to my system of the "*Arch Truss Girder*," where the curve of the arched chord is such,—the ties all being normal to it,—that the stress is uniform throughout the cord for a uniform horizontal loading throughout. The stress of the diagonals in the case there considered was compression.

That may be well where the girder is executed in wood; but if iron be employed it may be desirable that the diagonals resist tension. The origin was also taken at the crown.

I have found it more convenient to reckon the number of the pannel, or bay from one end.

In this article I propose to consider the case where the diagonals suffer tension; and to give rigorous analytical proof of every principle there stated, or here employed: and also, to reckon the number of pannel from the end of the truss.

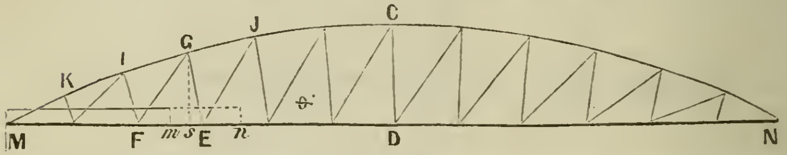
Dimensions of the Parts.

The formulæ given before would apply here; the only modification necessary being that required for reckoning the number of bay from the end.

Using the same notation as before, and taking the origin of co-ordinates at the point M in the Figure, we have

$x' = \mathrm{B} - x$, and $y' = a - y$; or $x = \mathrm{B} - x'$, and $y = a - y'$; which substituted in the expressions given before for the co-ordinates of K, I, G, &c., in the Figure, and dropping the accents give

$$\left.\begin{aligned} y_n &= \cos. i_n \left\{ a - \frac{\mathrm{B}^2}{4a} \sin.^2 i_n \right\} \quad . \\ x_n &= \mathrm{B} - \sin. i_n \left\{ \frac{\mathrm{B}^2}{2a} + a - \frac{\mathrm{B}^2}{4a} \sin.^2 i_n \right\} \end{aligned}\right\} \qquad (1)$$



When FIGE is the n^th pannel $n = 3$; the co-ordinates of G are x_3 and y_3; $\mathrm{GE} = l_3$, $\mathrm{GF} = \mathrm{L}_3$, $\mathrm{GI} = \mathrm{C}_3$, $\mathrm{ME} = b_3$,

$\mathrm{M}m = b'_3 = \dfrac{b_3 + b_2}{2} = \dfrac{b_n + b_{n-1}}{2} = b'_n$ and the angle at $\mathrm{G} = i_3$ the angle of GI with $\mathrm{ME} = i'_3$.

The angle of i_o at M is given by the formula.

$$b = \frac{\mathrm{B}^2}{2a} \sin. i. \qquad . \qquad . \qquad . \qquad . \qquad (2)$$

of the last article by making $b = \mathrm{B}$.

$$\therefore \quad \sin. i_o = \frac{2a}{\mathrm{B}}, \qquad . \qquad . \qquad . \qquad (3)$$

Hence for any angle i_n we have

$$i_n = i_o - \frac{n i_o}{\mathrm{N}} = \frac{i_o}{\mathrm{N}} (\mathrm{N} - n). \qquad . \qquad . \qquad (4)$$

Equation (2) gives the distance from the middle to any point.

$\therefore\ b_1 = \mathrm{B} - b$ or

$$b_n = \mathrm{B} - \frac{\mathrm{B}^2}{2a} \sin. i_n \qquad . \qquad . \qquad . \qquad (5)$$

for the distances MF, ME, &c.

The expression for l as before is

$$l_n = a - \frac{\mathrm{B}^2}{4a} \sin.^2 i_n . \qquad . \qquad . \qquad . \qquad (6)$$

The length of the diagonal FG is

$$\mathrm{L}_n = \frac{y_n}{\sin. \theta_n} = \Big((x_n - b_{n-1})^2 + y_n{}^2 \Big)^{\frac{1}{2}} \qquad . \qquad . \qquad (7)$$

in which $\tan g. \theta_n = \dfrac{y_n}{x_n - b_{n-1}}$. $\qquad . \qquad . \qquad . \qquad (8)$

The length of any part of the arched chord is

$$\text{c}_n = (x_n - x_{n-1}) \text{ sec. } i'_n,$$

in which $i'_n = \frac{i_n + i_{n-1}}{2}$.

Stress of the Parts.

The various parts of a truss built for any purpose, should be so proportioned as to withstand the greatest strain ever liable to come upon them. It is for this maximum stress that I have designed these formulæ for the truss.

If no other except a uniform loading throughout should ever come upon the arch truss, no diagonals would be necessary, nor would any compression ever occur upon the normals. This is the case theoretically, with the roof, but practically it is not. Snow and ice are liable to gather upon one side. This case would be represented more approximately by a load over part of it. And applied to railroad bridging the strength of each part is severely tested. Here the load may be regarded as uniform; and a long train moving over extends from the end upon which it enters, at different instances, to various parts along the roadway until we have the case of a uniform load throughout.

The stress of the diagonals being zero before the load enters, and zero after it entirely overreaches the bridge, and not being zero for a partial load, it is plain that each diagonal has a maximum as this load passes along.

I shall attempt to prove that the diagonal has its maximum as the end of the load reaches the middle of the bay to which the diagonal considered belongs: and that the chords have their maximum when the load extends entirely over.

Throughout this article in speaking of a loading, the uniform load as far as it extends is always referred to.

Now let the maximum stress of the upper or lower chord, at any points, as G or *s*, be required. Pass a plain through G, perpendicular to the straight chord. The two parts of the truss GEN and GFM being perfectly ridged may be regarded as single pieces. Then, according to principles demonstrated in "mechanics," there will exist in the upper chord a horizontal force equal and opposite to that in the lower chord, (since all the external forces are vertical) which will be the sum of the moments of the external forces taken upon either side of this section.

If the load extend, as represented in the Figure, a distance x beyond this section the sum of moments will be.

$$(\text{B}w + \text{V})\,\text{N}s - \text{W}x\,\frac{x}{2} = \text{H G}s.$$

Also by moments

$$\text{V} \cdot 2\text{B} = \text{W}\,(\text{M}s + x)\,\frac{\text{M}s + x}{2},$$

in which W = the weight per unit of the load,

w = the weight per unit of the truss,

and V = that portion of the vertical force at N resulting from the partial load; which substituted gives

$$H = \left(Bw + \frac{W}{4B}(Ms + x)^2\right)(2B - Ms)\frac{1}{Gs} - \frac{Wx^2}{2Gs} = \max. \qquad (10)$$

Placing the first differential co-efficient equal zero the first condition for a maximum gives $x = 2B - Ms$.

The second differential co-efficient $= - Ms$, which satisfies the second condition; that is, for a maximum of either chord the load must extend entirely over the truss. Hence

For the maximum strain of the upper chord we will have the expression $W\frac{B^2}{2a}$ or,

$$T = (W + w)\frac{B^2}{2a} \quad . \quad . \quad . \quad . \qquad (11)$$

This is also equal the maximum of the straight chord at the middle, but the ties diminish it toward the ends as given in the last article. Hence since $b'_1 = B - b'$ we will have for

Maximum strain of the straight chord

$$T = \frac{(W + w)}{2a}\left(B^4 - 4a^2(B - b')^2\right)^{\frac{1}{2}}. \quad . \quad . \qquad (12)$$

Tensile strain of the normals.

It is plain that the weight of the truss affects only the chords and normals, since from a uniform load there results no strain upon the diagonals.

The total tensile strain upon the normals from the middle to one end was found to be $P = \frac{wB^2}{2a}\ i_0$.

Hence the strain upon each for this case will be

$$\frac{P}{N} = (W + f\,w)\frac{B^2}{2Na}\ i_0 = p, \quad . \quad . \quad . \qquad (13)$$

in which f is a fraction expressing what part of the weight of the structure is contained in the roadway and straight chords. The value of p will be at its maximum when the load is entirely throughout, for the stress of the diagonals, being tension, tends toward diminishing that of the normals.

Stress of the diagonals.

The stress of the diagonals each have their maximum as the load in passing over, arrives at the middle of the bay considered. This strain being a function of the horizontal and vertical forces will have its maximum when the sum of the components of these forces in the diagonal have their maximum.

Let t_1 and t_2 represent these two components resulting from H and V respectively, h_1, h_2 the components in the chord.

Then, (referring to the figure,) the maximum of GF is for the load M m, upon the hypothesis assumed. If it be otherwise, let the load extend beyond m, a distance x. The expression for H is given in (10) by making $\mathrm{M}s = \mathrm{M}m = b'$, and neglecting w. This has its maximum for a load throughout, as has been shown. But the vertical force at the point considered is equal the vertical force at N, diminished by the opposite vertical force resulting from the load between m and M, or the transverse shearing at m.

Hence we will have

$$\mathrm{V} = \frac{\mathrm{W}}{4\mathrm{B}}\,(b'+x)^2 - \mathrm{W}x = \frac{b'}{4\mathrm{B}} + \frac{x}{4\mathrm{B}}\Big(x - 2\,(2\mathrm{B} - b)\Big). \tag{14}$$

which has its maximum for $x = 0$, V being at its maximum for a load only to the point considered, the sum of the components from V and H may have their maximum at this point.

Resolving the forces in the diagonal GF, and the part of the chord GI which together resist H and V we get,

$$h_1 \cos.\, i'_{\mathrm{n}} - t_1 \cos.\, \theta_{\mathrm{n}} = \mathrm{H}_{\mathrm{n}}\,,\ h_1 \sin.\, i'_{\mathrm{n}} = t_1 \sin.\, \theta\ ,$$

$$\text{and} - h_2 \sin.\, i'_{\mathrm{n}} + t_2 \sin.\, \theta_{\mathrm{n}} = \mathrm{V}_{\mathrm{n}}\,,\ h_2 \cos.\, i'_{\mathrm{n}} = t_2 \cos.\, \theta_{\mathrm{n}}\,,$$

from which

$$t_1 = \frac{\mathrm{H}\ \text{tang.}\ i'}{\sin.\ \theta - \cos.\ \theta\ \text{tang.}\ i'}, \text{ and } t_2 = \frac{\mathrm{V}}{\sin.\ \theta - \cos.\ \theta\ \text{tang.}\ i'}. \tag{15}$$

For a maximum of $t = t^1 + t_2$ we may omit the common denominator, and the expression that will give the maximum, will be $\mathrm{V} + \mathrm{H}\ \text{tang.}\ i$; or, substituting H and V as given above we will have

$$\left\{ \frac{\mathrm{W}}{4\,\mathrm{B}\,y}\,(2\,\mathrm{B} - b')\ (b + x)^2 - \frac{\mathrm{W}\,x^2}{2y} \right\} \text{tang.}\ i + \frac{\mathrm{W}}{4\mathrm{B}}\,(b'+x)^2 - \mathrm{W}x = \text{max.}$$

This may be transformed to

$$\frac{b'^2}{4\mathrm{B}y}\left\{ (2\mathrm{B} - b')\ \text{tang.}\ i + y \right\} - \frac{x}{4\mathrm{B}y}\,(b'\ \text{tang.}\ i - y)\ (x - 2(2\mathrm{B} - b')) \tag{16}$$

This will have its maximum when $x = 0$. For x being the only variable in the case, the first term is constant, and in the second term b' tang. $i < y$, and $x < 2\mathrm{B} - b'$ which makes the second term negative for any applicable positive values of x. I think no formula is necessary to show that t is no greater for a shorter load than b', since both H and V increase to the section considered;

That is, for a maximum stress of the diagonal, the loading must extend to the middle of the bay considered, and no farther.

The expression (16) does not hold for x negative, which corresponds to a shorter load than b'. For this case the moments must be taken upon the other side of the section. The conditions for a maximum from the differential calculus fail here.

The condition $\frac{dy}{d} = 0 =$ tangent, is satisfied only where an infinitessimal element of the arc constructed from $y = f(x)$ is horizontal. In the case above considered where $t = f(x)$ the ordinate t continues to increase from N to the section considered, and beyond. Consequently the tangent does not equal zero at this point. But there are points where it equals zero which would be indicated by the condition $\frac{dy}{dx} = 0$.

If the curve of the arched chord have any form whatever, concave toward the straight one; or if both are straight and parallel or not, the expression (16) holds equally true. For the extraordinary case in which the curve is convex toward the straight chord, b' tangent $i > y$, and the load must extend beyond the bay considered.

If the diagonal resist compression, the expression b' tangent $i < y$, is still true: or, the conditions for a maximum of t remain unchanged.

Therefore, the maximum stress of the diagonals for every *practical* case above mentioned, will be

$$t_1 + t_2 = t = \frac{V_n + H_n \text{ tang. } i'_n}{\text{sin. } \theta_n - \text{cos. } \theta_n \text{ tang. } i'_n} \quad . \quad . \qquad (17)$$

The weight of the truss having no effect upon the diagonals, the expression for H and V, to be used in this formula are those resulting from (10) and (14) by neglecting w and making $x = 0$. Or

$$V_n = \frac{W b'^2_n}{4B} \text{ and } H_n = \frac{2B - x_n}{y_n} V. \quad . \quad . \qquad (18)$$

Compression of the Normals.

The maximum compression of a normal occurs when the load arrives to the centre of the bay considered. Thus for the normal GE, the load extends to m: for FGJE may be regarded as a pannel, and the case comes under one already discussed above. Hence calling C the required compression and noticing that GE and GF resist H and V, the resolved forces become

$$c_n \text{ cos. } i'_n - h \text{ sin. } i'_{n+1} = V_n$$
$$c_n \text{ sin. } i'_n + h \text{ cos. } i'_{n+1} = H_n$$

from which

$$C_n = \frac{V_n + H_n \text{ tang. } i'_{n+1}}{\text{cos. } i_n + \text{sin. } i_n \text{ tang. } i'_{n+1}}. \quad . \quad . \qquad (19)$$

In all these formulæ the algebraic sign of the trigonometrical function of i must be observed.

These principles, for all kinds of truss bridges have been fully developed, and will probably soon appear before the public in a thorough and excellent work upon "Engineering" about to be issued by De Volson Wood,—Professor of Civil Engineering, University of Michigan.—Reference need only be had to his numerous articles published in our journals to insure all of the original, and genuine character of the work.

By means of a model constructed expressly for the purpose, he has been able to prove the results of his investigation of the truss by actual experiment, and to bring into the clearest light many errors existing in works now constantly referred to by the "*practical engineer.*"

Comparison of the new, and the ordinary parabolic systems in numerical results.

In order to show more clearly what I have obtained by these investigations upon the truss, I have computed by the preceding formulæ the value of the stresses of some of the principle parts for the two systems, which are tabulated below.

The quantities computed for the arch truss with parabolic arc, are the maximum values according to the same principles by which the new system has been treated.

The stresses of the diagonals acting as either ties or braces have been calculated for both systems. The example I have taken is for $2\text{B}=160$, $=$span; $a=20=$depth of truss; $2\text{N}=12=$whole number of bays; $w=0$; $\text{W}=1$; the same for both systems.

From this data I get for the compression of the arched chord throughout for this system, . .	160
The same at the middle of lower chord.	
At the end bay of lower chord, . . .	141·965
For parabolic system, arched chord at the crown, .	160
At last bay, ...	178·886
Lower chord throughout, . . .	160

THE DIAGONALS, AS TIES OR BRACES.

	NEW SYSTEM.		PARABOLIC SYSTEM.	
Bay.	as ties.	as braces.	as ties.	as braces.
2	16,480	20,220	19,524	16.500
3	20,222	21,919	20,905	18,078
4	21.111	23,786	22,686	20,488
5	23,247	24,926	23,864	22,502
6	24,615	25,183	23,811	23,766
7	25,098	24,532	23,710	24,185
8	24,650	22,986	22,322	23,673
9	23,245	20,617	20,140	22,302
10	20,893	19,227	17,402	20,125
11	17,686	14,286	14,700	17,395

It appears from these that the greatest stresses upon the diagonals are for those of the new system. The difference of the sums total of the two systems as ties is 8·183, and as braces 8·668. But this distributed among the ten diagonals are the hypothesis that an equal amount belongs to each, gives about 0·8. Or, the mean of the stresses being about 20, the increase of stress is about $\frac{4}{100}$, some more and some less. But at the same time there is a diminution in the total lengths of all the diagonals of about $\frac{1}{400}$.

But there is also an increase of stress in the normals, over the vertical ties of the parabolic form, of about $\frac{1}{20}$: but at the same time there is a gain in the length of about $\frac{1}{48}$ in favor of the new system.

Although the arguments founded upon the diagonals and normals against the new system slightly outweigh those in favor of it, still the great decrease of stress in the arched and straight chords must greatly overbalance them all. The diminution of stress for the arched chords near the abutments as may be seen above is 18·886, and in the straight chords 18·035, or a saving of about $\frac{1}{9}$ of the stress, or of area of section near the ends of the truss. These quantities vary with a or B.

If we consider the weight of the truss and make w nearly equal W, the amount of stress for the diagonals remains unchanged, *while the saving of stress to the chords is nearly doubled.*

In the "Howe's Truss" the stress of the chords diminish toward the ends. At the same time that of the diagonals is greatly augmented while the length remains the same. Probably this alone is sufficient to condemn the system for iron bridging. I have observed that iron bridges, under the arch truss system with parabolic arc, are quite common. But as iron gains the ascendency over wood as a building material for bridges,—*why is not this system with normal ties destined to become adopted as one of the most common forms?*

In the preceding part of my last article I stated, where integrating for expressions for co-ordinates to the curve, that I had not attempted the complete integration of the expression as a function of x and y only. I have since received a communication containing an algebraic solution of my final equations of $y=f(i)$ and $x=f(i)$ from Mr. E. P. Austin, Astronomer, formerly Assistant Astronomer to U.S. Lake Survey; he proceeded by transferring the origin of co-ordinates to the centre of the curve, transforming the trigonometrical functions into those of the cosine, placing the expressions $=0$, and eliminating by dividing one by the other repeatedly, until i disappeared. He gave the following for the form of the function,

$$a\,(x^2+y^2)^3+\,(b+cy^2)\ \ (x^2+y^2)^2+(d+ey^2+fy^4)\ \ (x^2+y^2)+gy^4+hy^2+i=0,$$

in which the quantities a,b,c, &c., are functions of a and ρ_0

This, he adds, is an equation of a curve which has a centre, because all the powers of the variables are of an even degree.

ERRATA TO LAST ARTICLE.

For third equation after (17) on p. 155, read $T'=\frac{WB}{2a}\left(\left(1+\frac{4y^2}{x^2}\right)^{\frac{1}{2}}\right.$

Equation (18) on p. 156, for sin. $\frac{-12a}{B}$ read, sin.$^{-1}\frac{2a}{B}$.

Result of Experiments on the Breaking Weight of Rolled Iron.

From the London Builder, No. 1110.

[From the paper by Mr. F. A. Paget, C. E.]

When we remember that the very best wrought iron of commerce is, to use the words of the well-known metallurgist, Saint-Claire Deville, but a metallic sponge, like platinum, the pores of which have been simply closed up by pressure or percussion; that, in one word, ordinary wrought iron has never as wrought iron, been fused, it will be seen that the uncertainties qualifying the material itself are still greater. Mr. Mallet thus found that, while the original hammered slab of a very large forged mass had a breaking strength of 24 tons to the square inch, it fell progressively to 17 and 16 tons at the different places of the mass, down to even as low as $6\frac{1}{2}$ tons in some parts. Unless this iron had been burnt, its tenacity could doubtlessly have been restored; and, if drawn into wire, its breaking weight might have been increased to perhaps 90 tons to the square inch,—at least before annealing. An average of 188 experiments, made by Mr. Kirkaldy on rolled bars, gave a maximum breaking strength of $30\frac{3}{4}$ tons, and a minimum of nearly 20 tons to the square inch. These influences of the manufactnre merely on the quality of wrought iron are almost independent of the chemical constitution of any individual bar. For instance, until it be proved to the contrary, there are many reasons for the general belief that the cold shortness of wrought iron is due to the presence of silicon and carbon, and its hot shortness to that of sulphur. A fractional percentage of copper also makes wrought iron hot-short. In truth, there are probably no two bars or parts of a bar, of an exactly similar chemical composition, or in an exactly similar state of molecular aggregation, and, therefore, of an exactly similar breaking strength or elastic limit. Even these are only a few of the elements of uncertainty in structural materials. But, when we further take into account the varied strains of extension, compression, distortion, twisting, and bending, to which mechanical structures are more or less subject; that the work done by a gradually applied load is doubled if this load be applied suddenly; that the impulsive strain of a moving load is generally more or less intensified by vibration; and that the varied shapes and arrangements intended to receive these strains must be often as much fixed by financial as scientific considerations, then the reason that the best engineering practice makes the ultimate strength of a wrought iron structure from four to six times the working load must be even popularly evident.

But these factors of safety are not sufficient. The structure must be tested as searchingly, and as far as is consistent with safety,—as far as is possible without injuring the material and its relation to the structure. In our case this limit is, in the main, given by the limit of elasticity of wrought iron under extension, as this limit is less for wrought iron than that of compression. It is also self-evident, that the mode of testing adopted ought to approximate as nearly as practicable with the kind of stress the object is intended to undergo in practice.

After loading a railway bridge with the greatest passive, or perhaps impulsive load that would ever come upon it in practise, the deflection and the permanent set, if any, are both carefully noted. As a boiler is subjected in practice to a complex train of mechanical and chemical forces that are always striving to break their bonds, its ultimate strength is made from six to eight times the working stress, and it ought to be periodically tested to half its working pressure. Its extension under this pressure is sometimes—and should always be—measured by the volume of water that is pumped in by pressure after the boiler has been filled; while the permanent set is determined by the difference between the volume pressed out by the contraction of the boiler when the pressure is withdrawn, and the volume of the water that remains in the boiler after the test—allowance being of course made for any slight leakages and sweating at the joints. In first class locomotive works, the deflexion and permanent set of the steel springs are always tested in an apparatus for the purpose. It may here be remarked that, although the designs of all the successful wrought iron structures ever built have been based on the assumption of a limit of elasticity, nevertheless the relation of the permanent set of wrought iron to its ultimate resistance under a given load, is still a subject of some discussion. We have, on the one hand, the testimony of Professor Eaton Hodgkinson, who says, that "the maxim of loading bodies within the elastic limit has no foundation in nature;" and, on the other hand, some appear to believe that iron is even improved by breaking it under at least a tensile stress. Mr. Hodgkinson found that a rod, 10 feet long, and of 1 square inch section, took a permanent set of 0·0005 of an inch under a static load of $1\frac{3}{4}$ tons. Mr. Edwin Clark obtained very similar results. Such a microscopic set, however, could be referred to the elongation caused by the heat generated by the internal friction of the particles, or to the probable fact that these bars were also new; and it is conceivable that they might have taken a slight permanent set, just as new ropes take a permanent set, without injury, when the strain is first applied. There are, indeed, very few forms of wrought iron in which its internal particles are not, *ab initio*, subject to some mutual strain. At any rate, these elongations were very slight, and increased uniformly up to tensions varying from about 10 to 15 or 16 tons on the square inch. Beyond these strains the bars elongated in an irregular manner, until they at last broke. At the same time, as Dr. Rankine remarks, the demonstration by Mr. E. Hodgkinson, that a set is produced by a strain much less than what would injure the specimen, renders the determination of the proof-strength a matter of some obscurity; but Dr. Rankine points out that the best test now known is the not producing an increasing set by the repeated application of a load. Some years ago, Mr. Loyd of Woolwich, made certain experiments which have been cited as proving that a breaking strain does not injure iron, even when this strain is four times repeated; or rather, that after breaking a bar into, say, two pieces, the two pieces are thereby made stronger. In, for instance, experiment 2, the $1\frac{5}{8}$ bar marked C was found to break with $33\frac{3}{4}$ tons, with a stretch of $9\frac{1}{4}$ inches in 54 inches; a piece of this bar then broke at $35\frac{1}{4}$ tons, with a stretch of only a quarter of an inch in 36 inches; another piece of the bar, 24 inches long, was broken at 37 tons, with a stretch of one inch; and at the fourth and last breakage was found to give way at $38\frac{3}{4}$ tons, but without any stretch at all. . . . . .

There is probably no metal the strength of which is influenced in such a remarkable way by temperature as iron. As M. Baudrimont showed in 1850, the tenacity of iron is less at 100° C. than at 0° C., but at 200° it is greater than at 0°, and these results have been exactly confirmed by Dr. Fairbairn in some experiments on boiler plates, communicated in a paper to the British Association. At yet higher temperatures this tenacity is of course diminished; and Seguin has shown that iron, the tensile strength of which could be represented by 100° at 10° C., had this tenacity lowered to 90·5 at 370° C., and to 58·7 at 500° C. In the royal dock-yards of Woolwich and Portsmouth the atmospheric temperature during the testing of each anchor or chain is carefully noted, although the proving houses themselves are kept at a mean temperature of 56° Fahr., by means of stoves, which also thus save the water pipes from freezing. This temperature of course falls a little during the winter and rises in summer, as the heat in the shade generally varies in England from about 76° to perhaps 34° Fahr. The action of frost on iron has not been completely investigated; and Dr. Percy recommends that some accurate experiments on the question be undertaken by the Institution of Civil Engineers. The daily observation of practical men has, however, as in so many other cases, preceded the deeper investigations of science. All workmen know that their tools, such as picks and chipping hammers, which have to undergo percussion in frosty weather, are then more liable to get broken. All chains are well-known to be more subject to snap under the same circumstances. There is always a notorious increase of accidents through breakages, both in the permanent way and rolling stock of railways during frosty weather. It is stated that during the severe winter of 1860-61, 498 rails were broken on the Chemin de Fer de l'Est, from the 11th December to the 31st January inclusive. No less than 258 were broken from the 21st to the 25th of January, during which period the thermometer descended to—7·8°, and even to — 16° Centigrade. General Morin relates that during the northern campaigns of the first empire, artillery veterans used to believe that wrought iron was subject to freezing, and after long winter bivouacs they never began their day's march without striking the gun-carriage axles in the direction of their length and the vibration thus produced was said to "thaw" the iron. An intense cold is also said to have enabled the French garrison of Hamburg to disable the cast iron siege guns by knocking off the trunnions before evacuating the place. Mr. Lenox stated, in evidence before the 1860 Committee, his belief that a cable would stand a test in warm weather that it might not in cold. The crews of the fishing vessels on the coast of Nova Scotia find that the cold renders their cables so brittle that a length of hempen cable is used for the portion out of the water while the anchor end is kept from the vicissitudes of the atmosphere by the usual average temperature of the sea. A few experiments made by Mr. Kirkaldy showed that the breaking strength of a bar is slightly reduced by freezing when a gradual breaking load was applied, but that this difference between a frozen and an unfrozen bolt is much more increased by a suddenly applied load, being 3 per cent. less when frozen. The usual way adopted by French engineers to test rails is, as we have seen, to prove a percentage of the lot by means of a falling weight. Some tests were carried out a few years ago by M. Couche, on a number of rails, of very good quality from the Anzin works. The monkey weighed 300 kilogrammes, and the distance between the supports was

1m. 10. When the thermometer varied from—4° C. down to—6° C., the weight had only to be raised, in an average of twelve experiments, to a height of 5 feet 6 inches, in order to break the rail; but when the thermometer rose from+3° to+8° C., then the weight had to be lifted for a fall of 7 feet 9 inches. Similar experiments conducted in 1860, showed that a difference of temperature from — 4° to + 5° Centigrade, was sufficient to greatly influence the height of fall necessary to break the rail. It is not unnatural to suppose that the particles of iron, after being worked at a heat and allowed to cool and set at a medium temperature, should, when that temperature is lowered, get into a state of mutual strain; or that any initial mutual strain should be thus intensified. The toys made of suddenly cooled glass, known as Prince Rupert's drops, are exaggerated instances of a similar action. The outside portions of a bar, of whatever size, would evidently cool, and consequently contract, first of all. The inside portions would also at last cool; but, having kept the outside portions distended, when the inside does cool, it then becomes a question to be determined by various circumstances, whether it would pull the outside shell into a state of compression, or whether the outside shell will draw the inside into fissures by tension. A somewhat similar explanation is given by Mr. Mallet of the rents caused in the interior of very massive forgings; and this state is probably always induced by the conditions of cooling in a small bar, but with, of course, a smaller range both as to size and temperature. In any case, it is apparent that a ductile, elastic material ought to be less affected by these doubtlessly complicated conditions of tensile and compressive strains. It is, therefore, probable that a hard, harsh, iron would be more affected by frost, than a soft ductile iron; and also, that the breaking strength of both qualities would be less affected by cold than their extensibility.

On Elastic Railway Wheels. By VAUGHAN PENDRED, Esq. C.E.

From the Lond. Mechanics' Magazine, May, 1864.

[Read before the Society of Engineers, May 2, 1864.]

The author commenced by stating that the subject of the paper he was about to read, "Elastic Railway Wheels," or, more strictly, certain expedients for imparting elasticity to these important members of the railway system, was one possessing much interest; indeed, he thought he was justified in saying that the adoption of certain mechanical expedients for seating tyres elastically on the wheels to which they belonged, afforded fair promise of enabling important changes to be introduced into the present system of constructing permanent way; while experience had shown already that by their aid, the duration of engine tyres might be nearly, if not quite doubled, especially on lines of rapid curvature, and that slipping or want of adhesion might be reduced to a minimum.

The paramount object had in view in the formation of a railway, was the reduction to a minimum of those forces which retard the motion of wheel carriages. Setting aside for the moment every other consideration, he might, therefore, define a perfect railway as consisting of two unyielding bars—parallel—absolutely hard on their upper surface at least, and so secured to the sub-structure on which they were ultimately supported, as to be incapable of moving or deflecting under the action of any practical load which they might be called on to sustain. Such a railway, the author stated, would not only reduce the amount of tractive force required for the propulsion of trains or carriages to the lowest limit, but would also possess elements of permanence, which are wanting in any track constructed with timber sleepers, cast iron chairs, &c., under existing arrangements; while, on the other hand, it would be open to certain objections, which have hitherto precluded its adoption.

After describing a theoretical railway track formed of girders laid down on stone blocks, the author went on to say that, were it but possible to introduce the use of stone sleepers, and to permit the ballast to settle its own differences with the sleepers after its own fashion, the expense of maintenance would be greatly reduced. Good ballast always had a tendency to consolidate and become hard, but, under existing circumstances, this quality which ought to be of all things the most desirable, is, on the contrary regarded as a great evil, and no sooner does the ballast become hard, and the timber manifest a tendency really to sleep, than the navvy stirs them both up with his pick. This we are told, is done to secure elasticity; but the ballast was not properly treated if compelled to become the elastic member of the system—to take a part for which it had no vocation, and could not properly assume. If it were not necessary that elasticity should be provided somewhere, a very excellent track indeed, might be made with stone blocks and rails bolted down directly to them, without chairs, and such a track would be infinitely more durable than any other involving the necessity for the use of wood in its construction, always provided that it were exempt from these destructive influences, to obviate the effects of which the elastic element is introduced; in other words, the author believed that the elastic element in our permanent way, as it is called, is the principal cause of its want of permanence, because wood rots and wears out, because nothing has been found as yet which is better than wood, under the given conditions, and because the mechanical arrangements are all so imperfect, and the bearing surfaces so small, that if that motion takes place among the parts which constitute an ordinary railway track which the element of elasticity is presupposed to permit, they quickly become loose, and the true rigidity on which the integrity of the entire structure depended, can only be approximately maintained by an excessive outlay of money and labor.

The author then explained at some length the nature of the action which a rapidly revolving wheel exerted on the rail beneath it, showing that elasticity prevented percussive action, and that the elastic element may be applied with more mechanical propriety beneath the tyre, than beneath the rail, and with equally good effects; and went on to say, that the percussive action of railway wheels traveling at speed is due directly to their revolution, and is almost independent of any true jumping whatever, and that elasticity operates more as a *preventive* of percussion than as a cure. In other words, the suddenness of strain characteristic of percussion was prevented, as he had just endeavored to show, instead of being absorbed or taken up, as might be the case, if the wheels were supposed to be lifted off the rail for a space and suffered to drop.

No means had yet been discovered for preventing the effects of percussion taking place between revolving wheels and the rail which supported them, but the introduction of elasticity, somewhere, into the system; and experience showed that this elasticity could be introduced with the utmost advantage into the wheel, or beneath the tyre, instead of, or in addition to, elasticity beneath the rail or the sleeper, and if no better expedients for securing this necessary element in track could be devised than those already in use, which depended for success on soft ballast, or costly destructible timber, then he believed it might yet be found advantageous to seat rails as rigidly as possible on heavy stone sleepers, and to adopt some expedient beneath the tyres of not the engine alone, but every wheel of the train, which should supply that elasticity which would, in such a case, be absent from the permanent way. This was, however, a point open to discussion, and he would therefore proceed to the consideration of the various expedients which had been proposed from time to time for seating tyres elastically, or in some one way imparting resilience in no ordinary degree to the wheels of vehicles. The idea of an elastic wheel was by no means new. Patent Office records showed that, with the first notion of propelling carriages by the adhesion of the wheels on which they rested, men sought to increase that adhesion by enlarging the surface in contact with the ground, either by using a very broad wheel, or by adopting certain expedients which would permit the wheel to depart slightly from a true circular shape, and become more or less oval.

The author then gave a short history of the elastic wheel theory, showing that Mr. T. Neville, the inventor of the multitubular boiler, had been one of the first to propose to use elastic tyres to obtain increased adhesion, as early as 1825. After describing many inventions, the author went on to say that all those expedients were indirect, and more or less unlike the arrangments adopted in modern railway practice; but he found that, in 1837, Sir George Cayley took out a patent which included nearly all that had been done since, in the idea at least, if not in the actual mode of application. Not only this, but he found that, in 1831, he had suggested, in the pages of the *Mechanics' Magazine*, an improvement in railway wheels, designed to reduce their wear, which possessed many points in common with the Griggs' wheel. Sir George writes, "If the wear and tear of railway conveyance be found too expensive, owing to the friction caused by such high pressure and great velocity, and that the use of springs to these carriages are not sufficient to remedy the evil, I think it probable that a dovetailed groove, filled with hard oak, driven in small pieces endways within the rim of the wheels, and then turned off in the lathe, might be serviceable and could be cheaply renewed; these pieces might be secured by a fox-wedge as commonly practised in similar cases." In Sir George Cayley's patent he describes a wheel "made with a deep flange, and to the opposite side of the tyre is secured a ring or annular plate of less depth than this flange; the space between the flange and the ring is to be occupied by a filling up of hoof or horn, or tough woods, or of other partially elastic substances, suitable for giving a slight degree of elasticity to the periphery, for diminishing the effects of percussion.

In 1839 Mr. W. Bridges Adams brought out a very ingenious elastic wheel, with spokes composed of steel rings, while the tyres were seated on wood blocks or felloes. Mr. Adams' wheels were tried under his own supervision, and at one time he had several sets running. The results obtained were not quite so satisfactory as he desired, principally because of certain practical difficulties met with in the construction of the wheel.

Setting aside one or two isolated instances of the use of elastic wheels, he found that Mr. George S. Griggs, locomotive superintendent of the Boston and Providence Railway, United States, was the first to seat the tyres of his driving wheels elastically on a practical scale. The means he employed to effect this purpose are very simple. American locomotives almost invariably have cast iron driving wheels, fitted with wrought iron or steel tyres. The Griggs' wheel was made with a number of transverse dovetailed grooves, cast in the periphery; into these grooves blocks of thoroughly dried, hard wood, such as fine-grained old oak or hickory, were driven, so that the grain of the timber ran parallel with the axle, and across the periphery of the wheel. When these blocks had been driven into their places, the wheel was put into the lathe, and so much turned off them that they only stood up above the surface, an eighth of an inch or so. The tyre, previously turned up true inside and out, was then heated and placed on the wheel, care being taken not to scorch the blocks on which it then bears, no metallic connexion of any kind existing between the wheel and its tyre.

The Griggs' wheel was brought out in 1857, and has been, and is extensively used in America, with the best results.

The "Canton," an engine on the Boston railway—as an example from many cited by the author—had run 133,373 miles; the tyres were now reduced to 1¼ inch in thickness. The four coupled drivers are 5 feet in diameter, and loaded with nearly 14 tons.

The thinness to which tyres seated elastically on Mr. Griggs' plan had been worn was very remarkable, and such as we dare not attempt on a rigid seating. Yet the fracture of an engine tyre on the Boston and Providence road is all but unknown.

Mr. Bridges Adams had lately introduced a very elegant arrangement. The rim of the wheel is turned slightly convex; the tyre is rolled with a groove on the inside. Into this groove two hoop springs, made in segments, and each about one-third of an inch, thick are placed, so as to break joint. These hoops are made of steel of the best quality. They bear only at their ends on the sides of the groove in the tyre, and the convex wheel rim, resting only on the centre of their breadth, is supported elastically. The tyre is secured in its place by a ring of iron sprung in at the back of the wheel. In a more recent modification the rim of the wheel is turned perfectly flat, and only a single spring hoop is used, thickened in the centre of its cross-section. This hoop is made in one piece, with the ends abutting, and forced on the wheel after being placed in the tyre by water pressure. The tyre of the first Adams' wheel, when unloaded, can be rotated on the wheel by hand, and a slight rocking motion of the wheel within the tyre is permitted, which enables the tyre always so adapt itself to the rail in the best possible manner. Wheels so fitted seldom or never slip. The area of surface between the spring and the tyre, and the spring and the wheel rim, is so great in consequence of the circular shape of the parts, that the wheel cannot turn with ease within the tyre when loaded; and this last, being elastically supported, yields slightly, becoming a little oblate, instead of remaining truly circular, and thereby acquires a better hold of the rail, consequent on an increase of bearing surface. Tyres usually break from tension ; but the tyres in this case, being elastically supported, can scarcely be said to be in tension at all, and it was, therefore, very improbable that they should break even in intense frost. Indeed, two of these tyres had been cut across for the purpose of experiment, and in that state actually did three days work drawing coal trains.

The first trial of Mr. Adams' spring wheel were made on the North London Railway, in 1858 or 1859. The spring tyres were applied to a set of disk wheels. The tyres were of Staffordshire iron, and ran a distance of 104,000 miles with very little wear. Low Moor tyres, rigidly seated on the same class of wheel, were completely worn out in running the same distance.

The next trial was on the Eastern Counties line, in the early part of 1859, when Staffordshire tyres were fitted, on the Adams' system, to the four coupled wheels, 5 ft. 6 ins. diameter, of a goods engine. The tyres were of the worst possible quality, and the springs very little better. These last broke and set so that they had to be removed, and thus far the experiment was a failure. In September 1859 however, a pair of Cooper & Co.'s tyres, elastically seated on the same principle, were applied to the leading wheels of a tank engine, working the Woolwich branch of the same line. This branch abounds in sharp curves, one of them being but 5½ chains radius, and another but 4¾ at the sharpest part. The engine had a 12 foot wheel base, and the load on the spring wheel was 7½ tons.

The engine began working on September 21st, 1859, and the wheels ran up to July 26th, in the following year, a distance of 25,240 miles before being turned up. The wheels were put under again in September, 1860, and ran up to January, 1861, a further distance of 8776 miles. At this time the tyres got quite loose from the breakage of one or two of the springs, and the engine requiring repairs generally, was taken out of work for a time, and the spring tyres were not replaced. The same class of tyres put on in the ordinary way require re-turning when they have run but 8000 to 10,000 miles, when the flanges are found much worn; afterwards they must be turned up every 4000 or 5000 miles until they are worn out.

Mr. Adams' tyres were largely used on the St. Helen's Railway, with great advantage. The author detailed at length the results obtained in the daily working of these tyres, showing that when Staffordshire iron was elastically seated, it actually gave a 3-5ths greater mileage than Krupps' steel rigidly seated.

The last form of elastic seating to which he would call attention had been recently patented by Mr. Mansell. The wheel rim is first turned up, and then the inside of the tyre is bored out, so that when put in place a space of about one inch intervenes between the two surfaces all round. This space is filled with a ring of wood—teak being used by preference—on to which the tyre is forced, and secured in its place by six segments of iron on each side of the wheel. A groove is turned in each edge of the tyre, and a similar groove in each edge of the wheel rim, into which raised ribs on these segments enter, while pins pass through the wood and secure the whole in place. These wheels have been so short a time in use, that many data do not exist about their performances yet. A good many of them were running on the South Eastern Railway. On the Great Eastern Railway one engine had the leading wheels fitted on this plan; they had been running some months, and showed little signs of wear.

An interesting discussion followed, in which Messrs. W. B. Adams, Z. Colburn, Ordish, Musey, and several other gentlemen took part. The paper was fully illustrated by diagrams.

On the Bursting of the Bradfield Reservoir. By HEWITT DAVIS.

From the London Builder, No. 1108.

I have read with much attention the evidence that has been given as to the cause of this sad accident, and must confess that I think as yet very little light has been thrown on it.

I have for many years been engaged as a draining engineer and government inspector in the drainage of clay land, and have had to observe the wonderful contractile power that clay exhibits in drying. I may instance the cracking of clay land in autumn, and our ability to make the strongest soil porous by inserting 4-feet drains every 27 feet. This experience leads me to think that this accident may have been caused by the drying and cracking of the clay puddle in the embankment, consequent upon the sinking of the water in the reservoir during this singularly dry winter, and its exposure while in this state to the sudden rise in the reservoir after the heavy rains at the time of the accident.

The reservoir was formed by an embankment carried across the valley, 90 feet high, for the purpose of collecting the water that came down in the wet seasons; and the embankment has been made watertight by having a puddle of clay carried up along its centre. This puddle, it appears, was made 14 feet thick at bottom; tapered off to 4 feet at top; and was supported, back and front, by the material of the embankment, which it is admitted was formed of loose earth; so that the puddle stood like a wall 90 feet high, 14 feet thick at bottom, tapering to 4 feet at top, exposed to wet and draft as the water rose and fell in the reservior. This puddle was made of clay worked wet, and would be dependent for its continuing imporous upon its retaining the water incorporated in it. Now, the question with me is, has this clay wall remained in a condition this winter to retain its moisture, and not undergone the change that takes place whenever clay parts with its moisture? I think not. The amount of rain-fall in the Midland Counties during the 6 preceding months to the accident had been remarkably small,—only 6 inches, or less than half the usual quantity,—and the springs throughout the county had been remarkably low. The ponds usually overflowing at Christmas were not half filled in February; and wells were dry that were never before known to fail at that season: so that the contents of the reservoir must have been reduced very low, and the embankment have been exposed to an unusual exhaustion of its moisture; and while in this condition the heavy rains came, and the water suddenly rose in the reservoir, filling the fissures under great pressure. The effect of this, I take it, has been that the water has been forced through the puddle. Let me ask, is this not likely to have been the case, and the cause of the accident?

MECHANICS, PHYSICS, AND CHEMISTRY.

On Ailanthine and the Ailanthus Silk-Moth.

From the Journ. Soc. of Arts, No. 572.

The following paper, by Dr. Robert Paterson (corresponding member), was read before the Botanical Society of Canada:—

There are few individuals who have not watched the interesting changes which takes place in the larvæ of the *Bombyx Mori*, or common silk-worm, from the point of its exit from the egg until it has reached its full butter-fly existence; and many there are who have been sadly disappointed at the mortality which comes over a brood of silk-worms in a single night from some cause or causes unknown, and consequently unremediable. Such epidemics are continually occurring in China as well as Europe, and constitute one of the greatest obstacles to the introduction of the culture of the silk-worm into England. What occasions this sudden decimation of these insects has never been determined, but has long led to a wish, on the part of those interested, that a more hardy breed of silk producing worms could be introduced into Europe, even though the produce was coarser, and of a worse color than the ordinary mulberry silk.

Recent information, through our missionaries in China, leads us to the knowledge that there is a considerable number of worms used by the Chinese, in different districts, for the production of various qualities and coarseness. These varieties of silk are used in China principally for the manufacture of dresses for the peasantry. Of late, however, some of these have reached this country, and have been considered durable and excellent. Could we but rear such silk in our country, as we hope shortly to be able to show that we can do, how much of the present overwhelming distress, which is visiting our manufacturing districts in consequence of the American war, might be avoided? Such material if not used along, might be mixed with cotton or wool, and thus many new and beautiful, if not durable, fabrics might be produced.

In 1814 Dr. Roxburgh* published an interesting memoir on the silk-producing moth of the East Indies, and soon afterwards the Arrindy or Palma Christi silk-worm was introduced into Europe. The castor oil plant, in this climate and in the north of France, is but a delicate shrub: in the south of Europe, however, where the temperature never reaches the freezing point, it becomes a tree of very striking aspect, with large and rich tinted foliage. In such districts, therefore, the Arrindy moth thrives well, having plenty of food, undergoing its changes rapidly, and yielding five or six crops annually of silk of excellent quality. What was required for our climate, however, was an insect which, while sufficiently hardy to stand our cold springs and autumns, would also be regardless of storms, rain, dew, &c. Such a worm was first sent to Europe by the Abbé Fantoni, a Piedmontese missionary in the province of Shan Tung. He sent some cocoons, immediately after the first gathering in 1856, to some friends in Turin. The name of the tree on the leaves of which they lived was to him a mystery, but he described it as being like the leaf of an Acacia: so when the young brood hatched, various and many were the plants tried for their food, until the leaves of the *Ailanthus glandulosa* were presented to them: these they immediately ate greedily, and always preferred them afterwards to any other kind of food.

There can now be little doubt but that the Arrindy or Palma Christi moth introduced into Europe from Dinajepore and Rungpore in Bengal, in 1854, and the Ailanthus moth introduced into Europe from the province of Shan Tung in China, in 1858, are one and the same animal. The insects introduced in 1854 were delicate, and did not stand much lowering of the temperature; besides, the tree on which they fed perishes at 32° or 33° Fah. The insects introduced in 1858 were hardy, stood rain and cold, and the tree which they preferred is a hardy one in our climate. Those introduced in 1858, from China, would not eat the Palma Christi, and very naturally it was believed that they were different insects; upon examination, however, they turned out to be the same. Their changes, the color of their larva, the character of the cocoon, the kind of silk, and the characterizing marks of the moth itself pronounce them at once to be the same animal. But how have these animals acquired such different habits of taste? This can only be explained on the supposition that a long period of hardening in a temperate climate, like the province of Shan Tung, would produce in course of time a more hardy progeny, feeding habitually on a common plant of the country, while the more effeminate brood of Central India preferred as food the leaves of a plant which will only flourish in warm latitudes. Unless specific distinctions exist, it is clearly a bad plan to distinguish an insect from the peculiar plant it eats, for this may be a simple point of preference—if it cannot get the one it will eat the other, and thrive on it; besides, a long period of hardening will often enable an animal to live and thrive on a vegetable very different from its native food. We need only instance the ordinary *Bombyx Mori*, or common silk-worm, the finest varieties of which, after passing a year or two in our climate, will live and thrive, and spin beautiful silk on the common lettuce. Of the tree on which the ailanthus worm feeds, it may be necessary here to speak shortly; we shall have to describe the animal itself more fully afterwards.

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*Linnæan Transactions, Vol. vii.

It appears that the tree was originally introduced into this country by the Abbé d'Incarville, in 1751, as the "Vernis de Japon" tree, or that which yielded the famous Japan or China varnish. This turned out, however, to be a mistake, as the true Japan varnish tree has since been introduced into Europe. Since this latter introduction, the *Ailanthus glandulosa* has been known as the false varnish tree. It is a hardy plant in our climate, standing severe winters well, and producing an abundant crop of leaves, especially from young shoots, in early summer. It has no especial partiality for particular varieties of soil, thriving as well, and producing as abundant a crop of leaves, in the most barren soil as in the richest loam. It seems equally indifferent, too, as to the characteristics of the atmosphere in which it lives, healthy young trees being observable in the squares and smoky environs of London. The advantages of a plant such as this in the rearing of a hardy animal on its foliage need not be pointed out. Throughout France, generally, this tree flowers and seeds freely, and the seed sprouts and grows readily in Great Britain; but in addition to this method of propagation, another exists in the roots, which if cut into pieces like the potato, spring forth and grow luxuriantly; no plant, indeed, can be more easily raised, or more easily increased when grown than the *Ailanthus glandulosa*. But to enable this plant when grown to yield a proper supply of food for the ailanthus worm, it is necessary to cut it down and grow it ozier-like. In this way young shoots spring forth abundantly, and bear large and delicate leaves fitted for the young worm, and greedily devoured by the older ones. They have an additional advantage also that when the insects are placed upon them in the open air they are more easily protected by nets, &c., from the depredations of birds, insects, &c.

So much for the plant on which the animal feeds. Let us now turn to the insect itself:—I have already stated that the ailanthus silk-worm was introduced into Europe in 1856. Its cultivators have not been idle since that time, as we find that M. Guérin Méneville endeavored to introduce this worm into France. His first experiment did not succeed, but the following year he reared a satisfactory crop of cocoons in the open air; this, however, and all the efforts of the Société d'Acclimatation of Paris were not sufficient to effect the general introduction of the animal into France. It became necessary for him to show that agriculturists might derive a profit, and a good one, from the rearing of this insect.

Energetic, and thoroughly convinced of the success of such an experiment on a large scale, he induced personal friends to experiment on a larger scale at Toulon, in Provence, and at Chinon (Indre-et-Loire), the one being nearly in the south, the other in the centre of France.

At Chinon, for instance, 4500 worms were placed upon flourishing thickets of ailanthus, which had been cut down and grown as bushes, with that intention. Their development progressed satisfactorily, and they yielded 3515 excellent cocoons, after suffering, without injury, rains, heavy storms, and the attacks of birds and insects. The result of the experiment was a loss of about a fourth part, while the average loss of mulberry silk-worms is about one-half.

M. Méneville, after some careful experiments and calculations, which were submitted to the imperial government, has thus stated his profit and loss account, on the rearing of ailanthine, or the silk of this worm, produced in districts south of Paris.

	Francs.
Twelve acres of ailanthus copse, share of expense of planting and annual expense of keeping up, ...	2030
Average of receipts from two crops of ailanthine, . .	9945

which leaves a balance of 7915 francs for the twelve acres, or in round English numbers, £330 for twelve acres, or £27 10s. per acre. In India and China there are said to be six crops of silk annually; in the south of France two or three crops, but in the north of France and Great Britain two at most, and more securely one crop might be relied on. Let us take one good crop, and see how our profit and loss account would stand in Great Britain. The half of £27 10s. or £13 15s. would be the result, or about it; and be it remembered, for land, that after the planting of the *ailanthus* it requires no manure or tillage whatever, and the kind of soil being that on which nothing else would grow, provided always that it has as sheltered and sunny an exposure as possible. It always occurred to me that the climate of Canada would be especially favorable for the growth of ailanthine. The insect and the plant on which it feeds will stand any amount of cold; and when the Canadian summer arrives, rapid growth would take place in the tree, followed by hatching of the worm; in this way food would be speedily produced for the young brood, and two, if not three, crops of silk taken from the trees during the season. The experiment is one worthy of trial.

In England and Scotland, for the last two years, some experimenters have been at work, but as yet without any quantitative result. In the spring of 1862 I received, through the kindness of a friend, fifty eggs of the *Bombyx Cynthia;* they hatched in about ten days after their arrival; they were fed with cut branches of *ailanthus;* kept in the ordinary temperature of the atmosphere, but under glass. From the fifty worms (for the eggs all hatched) with all my inexperience, I had thirty-five large and fine cocoons, being a result not far short of that in the central districts of France. With more experience, and with growing plants prepared for the trial, I do not fear for the result of a quantitative trial in Scotland at any future year.

It is my intention, in describing this insect, to follow the different changes which it undergoes from the egg onwards until we arrive at the characteristic moth itself, from which distinctive marks and peculiarities are chiefly taken.

The Eggs.—These are about the size of a large pin head, twice as large as those of the mulberry silk-worm, with which we are all familiar. They are yellow-colored, equally large at both ends, flattened from above downwards, and with a depression in their centre. They soon change their color to a greenish-black, the color becoming more marked the nearer the point of hatching is at hand. The caterpillars are hatched from ten to fifteen days after the eggs are laid, according to temperature.

The Caterpillar.—When the worm first escapes from the egg it is exceedingly minute; the color of the segments of its body at this early stage is obviously yellow, but there are so great a number of black spots and dark colored tubercles over it, as to give the impression that it is of a black color; during the second period, that is to say after the first change of skin, the yellow color becomes more marked, but the spots and tubercles are still black. During the third period they become nearly pure white, arising from the presence of a white mealy secretion over their bodies, destined, obviously, to protect them from rain or dew, as water will not fix on it; the spots and points of the tubercles are still black or bluish black.

During the fourth period the body is at first white, but gradually changes to a pale green, the tubercles assuming the same color, and soon the head, the feet and the last segment become of a golden yellow; the flowery secretion still, to a certain extent exists and there are always black points upon the segments or rings of the body.

During the fifth period the emerald green coloring becomes more intense, the points, as to segments, assume a soft black color, and the extremities of the tubercles a marine blue. The caterpillar grows rapidly during this stage, eats largely and greedily till it attains the length of from $2\frac{3}{4}$ to 3 inches long; it then ceases to eat, becomes torpid for a few days, and after fastening a few leaves together at the extremity of a leaf or branch, it begins its cocoon. Such is the general character of the changes which this caterpillar undergoes; but to enable those who may follow out this inquiry to know when these changes may be expected and the size of the animals in them, I will give a short table of my own experience, and that of my friend Dr. Gudwad, both in Scotland:—

Eggs hatched, 28 to 30th June,	.	.	size $\frac{1}{4}$ of an inch.
First change, 7 to 9th July,	.	.	" $\frac{1}{2}$ "
Second change, 13 to 15th July,	.	.	" 1 "
Third change, 20 to 22d July,	.	.	" $1\frac{1}{4}$ "
Fourth change, 28 to 30th July,	.	.	" $1\frac{1}{2}$ to 2 inches.

From this time till the period when it begins to spin it rapidly grows till it reaches from two-and-a-half to three inches long, depending upon the abundance and quality of its food.

The experience of my friend Dr. Gudwad is as follows:—Eggs hatched 19th September; 28th September first change began; 5th October second change began; 12th October third change began; 21st October fourth change began; 3d November began cocoon. The temperature ranged from 47° to 55°.

The Cocoon.—I have already remarked that after a short period of torpidity when no more food is taken, and during which the remains of the undigested food are passed by the worm in abundance, it begins its cocoon by fastening some threads of silk to the end of the branch or leaf stalk, and after binding some leaflets together, it spins its cocoon in the hollow thus formed. The color of the silk is of a yellowish-brown very like, indeed, to that of a decayed leaf. In weaving its cocoon the worm leaves at its lower extremity an elastic opening for the exit of the moth. The threads at this opening are not cut across, but simply turned and laid one over another. The silk of this worm has not as yet been unwound in a continuous thread; this, doubtless, arises from the substance which glues the threads together, requiring some other solvent than the warm water which so readily effects the solution of the gummy secretion of the mulberry silk. This however, cannot long remain undiscovered in this country, as a chemical solvent for this secretion will doubtless ere long be found.* In China even there is reason to believe that this has been accomplished, as the last examples of *ailanthine* from that country are stated to leave no doubt of their having been unwound from the cocoon. Even the carded silk of this worm is abundantly used. In China it forms the most durable dresses of the peasantry, dresses which are often handed down from father to son. In France this "flosille" or floss is abundantly used for weaving with thread and wool and in the manufacture of fancy stuffs. At Roubaix, Nismes, and Lyons, it is imported from abroad in large quantities to the extent of 1,290,000 kilogrammes annually.

Mons. Geoffrey St. Hilaire, President of the Société d'Acclimatation of Paris says:—" Here is the report of the weavers at Alsace, who have made use of ailanthus silk. M. Schlumberger has found the cocoon very easy to card and spin; the thread obtained is less brilliant, strong and rough; it left no residue, not more than in combing the thread. It is most excellent stuff for use in all manufacture where *burre* is employed. The cocoons are easily cleaned, and they will take a good dye. This culture made on a great scale, will furnish in abundance a finer and stronger floss than the mulberry silk-worm. The worm remains in the cocoon in the chrysalis condition for from twenty-six to thirty days, at which time the moth makes its appearance, coming quickly and easily through the valvular opening at the extremity of the cocoon. At this time its wings are moist, soft, and folded up;

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*It has been stated by some that the cause of the silk not winding off results from the slanting opening at the bottom of the cocoon, admitting water, and thus sinking it and breaking the thread. This explanation is not satisfactory and is inconsistent with fact.

and, naturally, upon emerging from the cocoon, it seizes hold of the lower part of it, thus allowing its large wings to drop, become unfolded and stiffen. If this precaution is not taken when the moths are allowed to exit artificially, their wings never expand, but remain crumpled up, the moth never regaining much activity with its wings in this state, and seldom connecting itself with the opposite sex. In rearing these moths, therefore, it is of consequence to observe that upon their exit from the cocoon they have some substance on which they can climb up and allow their wings to hang down and become expanded.

The moth has been long familiar to us, in collections of Chinese butter-flies, brought to this country. It is large, the expansion of its wings being about five inches; the head and antennæ are greyish-brown, the latter strongly pectinated; thorax and abdomen lighter-grey; wings with a broad transverse light-colored band near the middle, the space within which (forming nearly an equilateral triangle) is brownish-grey, and that without ash-color, running into brownish-grey at the margins of the wings. Just within the margins there are two narrow brown streaks running parallel with them, somewhat interrupted before reaching a black spot near the apex of the superior wings; this spot is surmounted by a white crescent, and a zigzag white line runs from it to the tip. The basel portion of the superior wings is traversed by an ash-colored bar commencing on the posterior edges next the shoulder, and after continuing in nearly a straight line for about half-an-inch is suddenly deflected and terminates on the anterior margin; between this bar and the transverse scapula line there is a pale longitudinal spot surrounded with black. The under wings likewise bear a similar spot but more crescent-shaped, and towards their base there is an ash-colored arched bar bounded on the outer side with black. The under side differs principally in being paler and destitute of the angular and arched bars at the base of the upper and lower wings.* These moths when in health and especially in sunshine, connect themselves and lay eggs in a few days. If they do not develop their wings, or the temperature is low and without sunshine, the males do not seek after the females, hence the eggs laid are often, under these circumstances, unproductive.

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* Sir H. Jardine's description of *Saturnia Cynthia*, and corresponding in every particular to *ailanthus* silk-moth.

Preservation of Wood.

From the London Chemical News, No. 227.

The following composition is recommended to protect the bottom of posts, pailings, and tubs set in the earth:—Forty parts of chalk are added to fifty parts of resin, and four parts of linseed oil, melted together in an iron pot. One part of native oxide of copper is then added, and one part of sulphuric acid, is cautiously stirred in. The mixture is applied hot with a strong brush, and forms, when dry, a varnish as hard as stone.—*Neues Jahrb. für Pharm.*, bd xx., s. 235.

Steam-Boiler Explosion at Messrs. Cornelius & Baker's Manufactory, Philadelphia—Verdict of the Coroner's Jury.

In accordance with your (Coroner Taylor's) instructions, we, your jurymen, have examined carefully the premises of Messrs. Cornelius & Baker, on Cherry Street, the scene of the late disastrous explosion: have heard the testimony of the witnesses brought before us, and, having exercised our best judgment in the consideration of the case, are now prepared to render our verdict, prefacing it by a statement of the facts as presented to us, and detailing our deductions from all the evidence variously accumulated.

FIG. 1.

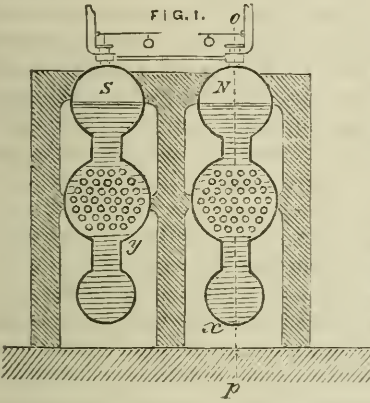
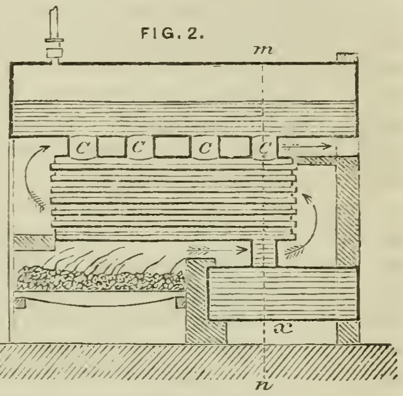


FIG. 2.



The boilers which exploded were designed* and built by Messrs. Morgan & Orr, of Philadelphia, and had been in use about two and a half years. There were two of these boilers set side by side, but working independently of each other; of these, the one towards the south was built and put in operation a few weeks before the other, which we may call the north boiler. Each of these boilers consisted of three parts, or three cylindrical boilers, placed horizontally one above the other, and called, in the order of their superposition, the mud-drum at the bottom, then the tube-drum, and on top the steam-drum. The mud-drum was back of the grate bars, and was 24 inches in diameter and 6½ feet long. The tube-drum extended over the fire, and was 41 inches in diameter and 10 feet long. The steam-drum was 36 inches in diameter and 14¼ feet long. The water line was at the middle of the steam-drum, which part was therefore half full of water: the tube-drum and mud-drum below this were of course, intended to contain water only.

The mud-drum was connected with the tube-drum by one neck 12 inches in diameter and 12 inches long, while the tube-drum was connected with the steam-drum by four necks each 12 inches in diameter and 8 inches long.

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* By this word, the Jury did not mean to say that the boiler was originally designed or devised by Messrs. Morgan & Orr, (for it was in testimony before them that the original design or arrangement was by Mr. Gavitt,) but that it was selected or designated for Messrs. Cornelius & Baker by Messrs. Morgan & Orr. Ed.

The tube-drum contained 58 tubes of wrought iron each 3 inches internal diameter, running from end to end; through these the fire passed, the water surrounding them. There was thus left water spaces between the tubes themselves, and between the outer row of tubes and the shell of the boiler of only 1 inch in each case.

The fire from the furnace was made to pass under the tube-drum and over the mud-drum to the back end of the former, then returned through the tubes of the tube-drum to the front of the boiler, and then passed over the upper half of the tube drum and under the steam-drum to the chimney.

Water was supplied to the boilers by a No. 5 Giffard injector, through pipes passing from the front of the boiler to the back of the mud-drums, which they entered at the lowest part. Water was supplied to each boiler separately, and cocks were arranged to control the filling of each boiler independently. There was no water connexion from boiler to boiler other than these feed-pipes, which in practice were not used to connect the boilers, but each one was fed separately.

The steam was taken from the front ends of the steam-drums by cast iron pipes. To each boiler over the pipe was attached a safety-valve, and the steam passed to the engine through a single pipe, but could be controlled by both cocks, so as to enable each boiler to be used independently of the other. To determine the water level, each boiler was supplied with three try-cocks: one at the working water level—*i.e.* at the centre of the steam-drum—one four inches above and another four inches below this point. There were no other means of observing the water level beside these cocks: and the lowest of them stood nine feet from the floor, and could only be reached by means of a rod or by climbing up to it.

The firm seemed to have placed great reliance upon steam-pressure-gauges, one of which was situated in their office in a conspicuous place, while the other was near the engine: but so far removed from the boilers that it could not be seen by the engine tender when he was at the boilers, the engine and boilers being at least fifty feet apart. Thus the engine tender had no means of judging as to his steam-pressure when working at or near his boilers.

From the evidence presented to us we learn that some three months previous to the explosion, the mud-drum under the south boiler was found to leak in one of its cast iron heads and was consequently removed and sent to Messrs. Morgan & Orr for repairs. But upon examination, they (finding the iron very thin from corrosion) notified Cornelius & Baker of the fact, and the firm ordered a new mud-drum to be put up in place of the old one.

The mud-drums of both boilers were said to have been made from pieces of boilers which had been previously in use for two years, but which were considered to be in good condition, and, in the opinion of the builders, better than new iron. We are surprised that, at the time of the substitution of the new drum, there was no rigid examination into the condition of the mud-drum under the north boiler, which was also made of old iron, and had been in use nearly as long as the one which had given out.

The mud-drum under the north boiler at the time of the explosion was therefore about the same age, or had been in use about as long as the old mud-drum under the south boiler, and after the explosion was found to be very badly corroded on the inside, the iron in some places being less than one-sixteenth of an inch in thickness; this in places where there seemed to have been no outside rusting of the drum. The outside was also found to be much corroded in parts which had been in contact with the walls.

The engine-tender has been examined, and seems to understand his business, and no testimony has been submitted to us that would in the least degree tend to show any habitual neglect of his duties, but, on the contrary, his character for sobriety and close application to his work seems fully substantiated.

From his testimony, corroborated by that of other witnesses, we learn that on the morning of the explosion he was behindhand with his steam which was not up to its working pressure when the engine was started.

He was not putting any water into the boiler at the time of the explosion, nor for some time before. The only testimony we have of the height of the water at this time is from the engine-tender himself. He states that he had a full supply of water, and that he tried the cocks just before leaving the boiler-room. Other witnesses were in the boiler-room a few minutes previous to the explosion, and had there been any water indicator which could have given a visible indication of the water level, their testimony would have been of the greatest value.

The engine-tender was away from his post at the time of the explosion, and no competent man had been left in his place. The only instructions were to a workman in the stamping-room near-by to watch the steam-gauge, which was in the engine room out of sight of the boilers, as before mentioned, and your jury do not think that the man to whom the direction was given was a competent person to take charge of the boilers.

In the absence of any evidence of gross neglect on the part of the engine tender, and assuming that the water was at its proper height a few minutes before the explosion, and that the steam-pressure was moderate, as was proved by many witnesses, we must direct our attention to the probably weak parts of the boiler, in looking for the cause of its disastrous explosion.

The mud-drum under the north boiler has shown itself to be less than 1-16th of an inch in parts which did not break, and was presumably weaker in parts destroyed by the rupture. No one can maintain for an instant that it was in a fit condition to bear the ordinary working pressure.

We believe that this was the first part to give out, but as the bursting of a mud-drum does not always cause an explosion of the boiler, and as the violent explosion in this case seems to have occurred in the tube-drum of this north boiler, which was literally torn to pieces, we must infer that, as the water fell, by reason of this primary rupture in the mud-drum, to a dangerous level, the water was driven away from the tubes by excessive ebullition; that thus some of the tubes being bare and unduly heated, the water temporarily driven away returned, and coming upon the heated tubes, caused the explosion.

We believe the explosion of the south boiler to have been the result of injuries inflicted by the bursting of the north boiler.

With regard to the form of the boiler and its liability to explosion, your jury believe, from the weight of the expert evidence brought before them, that this form of boiler is not safe, and that, by excessive firing and even moderately low water, (we mean before the water has fallen so as to uncover any of the tubes.) the water can be driven away from parts of the tubes, laying them bare for a time: and that, upon any slacking of the fire and the return of the water upon the tubes, an undue strain will be brought to bear on the boiler.

We think that the boiler is constructed on erroneous principles, for the following reasons: Granting that the cylindrical form of each part gives strength, the cylinders are materially weakened by the holes made for the connecting necks.

We hold, too, that the mode of applying the heat is entirely wrong. The greatest heat of the fire comes in contact with the under side of the tube-drum at its front end; it then, at a gradually decreasing temperature, passes upward in the system through the tubes, and thus through the very water put in motion by the intense heat at the lower part of the tube-drum, and finally passes away in contact with the steam-drum above. The capacity of water to take up heat from the products of combustion is, as we know, proportional to the difference of temperature in favor of the latter. In other words, water at 80° can take up more heat from products of combustion at 300°, than water at 300° can.

To use the heat, therefore, to the best advantage, the products of combustion should be made so to pass through the boiler system as to bring the gradually cooling gases in contact successively with cooler portions of the water. We express this opinion only to show that we do not think this boiler has any claim for economy to compensate for its complicated form, and we judge that a single cylinder boiler partly filled with tubes, with the water-line in the same cylinder, would be at once safer and more economical than this form of boiler. We are aware that many of these boilers are in use, and we would urge the greatest caution in their management. They may be moderately safe when the boiler capacity is large as compared with the work to be done, but are not safe where the consumption of steam requires heavy firing to keep up the pressure. Living as we do among steam boilers, knowing that in our most crowded thoroughfares they are under the very pavements, it is due to the safety of the community that strict municipal surveillance should be kept over the condition and the structure of the boilers used. We would, therefore, in the strongest terms urge the action of the authorities in reference to the use of steam.

There seems now to be no system of inspection in existence. Any form and any condition of boiler may be put in use, and when an explosion occurs, the fact that so many similar boilers have not exploded shields from blame those who would justly be held accountable for the destruction caused.

Messrs. Cornelius & Baker, are like thousands of others using steam power, not professional engineers; they therefore depend upon the advice of others in regard to all matters connected with their steam generating apparatus. Their responsibility would seem to rest with the choice of advisers and with their close supervision of those under them in responsible positions. We believe that they have in the latter case been careful in selecting an engine tender, and watchful over his actions; we can find no testimony to impeach the sobriety or competency of this engine tender; but we believe he has not made as careful and as frequent examination of the internal condition of the mud-drum as he should have done; but in this he is not singular; we have heard, and are hearing daily since the explosion, of mud-drums giving out in various parts of the city, and the warning has led to an examination of others which, although they have not exploded, are too thin to be safe.

All this points to the necessity of some reform in the laws regulating the use of steam; competent inspectors, free from all political influence, should be in charge of the matter, with power to call on the best engineering talent of the country for advice: and laws should be passed regulating the number and character of danger indicators which should be attached to each boiler as is customary in France and other countries.

For the safety of our citizens it has been found necessary to provide building inspectors, and to pass laws governing the thickness of walls and the arrangement of buildings, but no law is brought to bear upon the regulation of steam power.

The sooner this is done the better for the community at large.

We therefore render the following verdict. That on the 25th of April, 1864, William Bartholomew, Alfred Schaffer, Samuel Davis, Thomas H. Albertson, George Hess, J. L. Snyder, John Porter, and Anthony S. Fry, came to their death by reason of injuries inflicted by the explosion of a steam-boiler in the establishment of Messrs. Cornelius & Baker, Cherry Street, below Ninth, Philadelphia, the primary cause of said explosion being the weakness of a part of the boiler known as the mud-drum.

COLEMAN SELLERS,	JOHN F. FRAZER,
R. E. ROGERS,	HENRY MORTON,
JOHN W. NYSTROM,	SAMUEL J. CRESSWELL.

Why Bees Work in the Dark.

From the London Chemical News, No. 230.

A lifetime might be spent in investigating the mysteries hidden in a bee-hive, and still half of the secrets would be undiscovered. The formation of the cell has long been a celebrated problem for the mathematician, whilst the changes which the honey undergoes offer at least an equal interest to the chemist. Every one knows what honey fresh from the comb is like. It is a clear yellow syrup, without a trace of solid sugar in it. Upon straining, however, it gradually assumes a crystalline appearance—it *candies*, as the saying is—and ultimately becomes a solid mass of sugar. It has not been suspected that this change was a photographic action. That the same agent which alters the molecular arrangement of the iodide of silver on the excited collodion plate, and determines the formations of camphor and iodine crystals in a bottle, causes the syrupy honey to assume a crystalline form. This, however, is the case. M. Scheibler has enclosed honey in stoppered flasks, some of which he has kept in perfect darkness, whilst others have been exposed to the light. The invariable result has been that the sunned portion rapidly crystallizes, whilst that kept in the dark has remained perfectly liquid. We now see why bees are so careful to work in perfect darkness, and why they are so careful to obscure the glass windows which are sometimes placed in their hives. The existence of their young depends on the liquidity of the saccharine food persented to them, and if light were allowed access to this the syrup would gradually acquire a more or less solid consistency; it would seal up the cells, and in all probability prove fatal to the inmates of the hives.—*Chronicle of Optics*, "*Quarterly Journal of Science.*"

For the Journal of the Franklin Institute.

Working Steam Expansively. By W. Sewell.

It is a matter of public notoriety that nearly all the screw steamers of the British Navy (and of the British mercantile marine, too, it may be added) use the lap slide-valve and Stephenson link (without an independent cut-off valve,) cutting off the steam between $\frac{5}{8}$ and $\frac{7}{10}$ of the stroke of the piston, that is expanding it through $\frac{3}{8}$ and $\frac{3}{10}$ of the stroke. Of course, with this arrangement the steam can be worked still more expansively at the will of the engineer, by simply raising the link. In this manner the steam can be cut off properly at $\frac{3}{8}$ths the stroke, allowing it to expand through the remaining $\frac{5}{8}$ths. This is precisely similar to the valve-gear now used on all the navy steamers, and almost universally on locomotives; an independent cut-off valve being virtually discarded from all engines running at high speed.

Since economy in the cost of steam power is of as much importance in the British Navy as anywhere else, it is not to be supposed that the British Government adopted this valve-gear, and a point of cut-off at $\frac{5}{8}$ths the stroke of the piston, without satisfying itself that no increase of economy was to be obtained from complicated valve-gear, working independent cut-off valves, and using steam at high grades of expansion. On the contrary, it has experimented extensively, and established practically and incontestibly the practical superiority of the former system.

One of these experiments alluded to was made with the magnificent line-of-battle screw ship "Duke of Wellington," of 3759 tons burthen, and carrying 131 guns. The engines are geared: their cylinders, two in number, have a diameter of 94 inches, and a piston stroke of 54 inches.

With this machinery five experiments were made, cutting off the steam respectively at $\frac{6}{10}$ths, $\frac{1}{4}$th, $\frac{1}{5}$th, $\frac{1}{14}$th, and $\frac{1}{30}$th of the stroke of the piston. The results will be found in the following table:

Cut off.	Steam in Boilers.	Vacuum.	Throttle.	Revolutions of engine per minute.	Indicator pressure on pistons.	Indicated horse power.	Coal consumed per hour.	Pounds of coal consumed per hour per indicated horse power.
	lbs.	in.			lbs.		cwt.	
$\frac{6}{10}$	13	27	wide open	30½	18·98	2176·2	70.	7·205
$\frac{1}{4}$	13	27	"	28	13·95	1468·4	45.	6·865
$\frac{1}{5}$	14	27¼	"	26	12.80	1251·1	38.	6·804
$\frac{1}{14}$	14	27¼	"	24	9·30	839·1	26.	6·941
$\frac{1}{30}$	14½	27¾	"	20	8·20	616·5	22.	7·993

The annexed Indicator Diagrams, Plate V, were taken during the trials.

No. 1.

Mean of the two figures,	18·98 lbs.
Steam,	13
Vacuum,	27
Revolutions,	30½
Consumption of Coal,	70 cwt. per hour.
Throttle-valve wide open. Speed,	10·2 knots.

No. 2. First grade of expansion.

Mean of the two figures,	13·95 lbs.
Steam,	13
Vacuum,	27
Revolutions,	28
Speed of ship,	9 knots.
Consumption of Coal,	45 cwt. per hour.

By losing a trifle over one knot per hour, we saved 25 cwt. of coal in the same time by working on the first grade of expansion.

No. 3. Second grade of expansion.

Mean of the two figures,	12·80 lbs.
Steam,	14
Vacuum,	27¼
Revolutions,	26
Speed of ship,	8½ knots.
Consumption of Coal,	38 cwt. per hour.

No. 4. Third grade of expansion.

Mean of the two figures,	9·30 lbs.
Steam,	14
Vacuum,	27¼
Revolutions,	24
Speed of ship,	7½ knots.
Consumption of Coal,	26 cwt. per hour.

No. 5. Fourth grade of expansion.

Mean of the two figures,	8·2 lbs.
Steam,	$14\frac{1}{2}$
Vacuum,	$27\frac{3}{4}$
Revolutions,	20
Speed of ship,	$6\frac{1}{2}$ knots.
Consumption of Coal,	22 cwt. per hour.

From the above table it will be seen that the cost of the power in fuel when cutting off at $\frac{1}{5}$ the stroke, (the most economical point in these experiments) was $5\frac{1}{2}$ per cent. less than when cutting off at $\frac{6}{10}$ the stroke, but that when the expansion was carried out to cutting off at $\frac{3}{10}$ the stroke, the cost of power in fuel was 11 per centum more than when cutting off at $\frac{6}{10}$ths. When, however, the conditions of the experiments are examined, the reader will have no difficulty in reaching the conclusion that the $\frac{6}{10}$th point of cutting off was *considerably* the most economical. In the first place, an inspection of the indicator diagrams taken on the occasion, shows that the back pressure with the $\frac{6}{10}$ point of cutting off was greater than with any other point, owing to the smallness of the cylinder ports, which did not allow the larger mass of steam to be evacuated as promptly as the smaller masses with the other points of cutting off. This was purely a fault of proportion in the machine, which alone, if corrected, would have increased the economical effect with the $\frac{6}{10}$ cut-off more than five per cent., and placed it on an equality with the best of the others. Again, the vessel was very short in boiler power, and to maintain the pressure at 13 pounds, the fires had to be forced to the utmost when cutting off at $\frac{6}{10}$; while with the reduced speed of engines and vessel, when cutting off at the shorter points, the combustion of the fuel was comparatively slow, ranging from $\frac{1}{3}$ to $\frac{2}{3}$ only of the maximum. The shorter points of cutting off appear, therefore, in the table, with all the advantage due to slow combustion credited to the expansion, yet notwithstanding all these advantageous circumstances in their favor, the result shows no gain for the expansion. The considerable cost of the power in fuel throughout all these experiments is due to the faulty plan and proportions of boiler employed, the quality of the coal, and the excessive rate of the combustion in the experiment when cutting off at $\frac{6}{10}$, which, as the grate surface was $467\frac{1}{2}$ square feet, reached 33·54 pounds per hour per square foot. In the experiment cutting off at $\frac{6}{10}$, the steam jet was powerfully used to maintain this combustion, and all the steam expended in it counts in the table against that point of cutting off, while in the experiment with the $\frac{1}{5}$th cut-off the rate of combustion (18·21 pounds of coal per square foot of grate per hour,) was maintained by the natural draft. The cut-off valve, except for the $\frac{6}{10}$ point, was an independent adjustable slide; for the $\frac{6}{10}$ point the lap on the main slide was used.

The foregoing information was obtained from Inspector of Machinery, James Steil, R. N., the very able chief engineer of the vessel who conducted the experiments. The results, never before published, are

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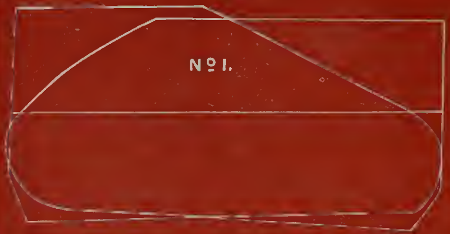
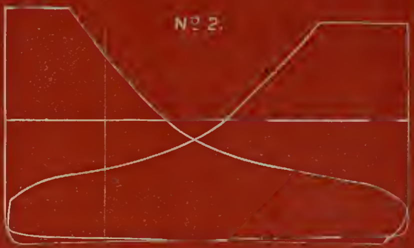
INDICATOR DIAGRAMS

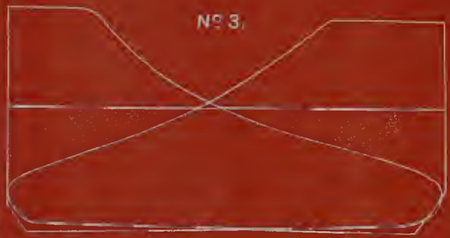
From the Engines of H.B.M. Steamer Duke of Wellington.

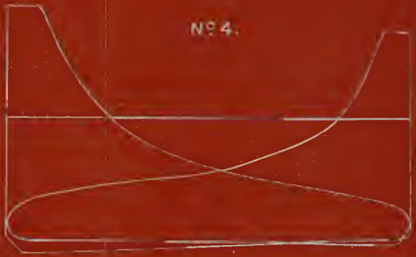
Taken during the Experiments on Expansion.

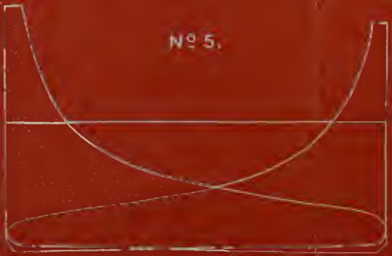
Scale. 10lbs. to the inch.











remarkably in harmony with those of the numerous, elaborate, and extensive experiments made by our own Navy Department upon the same subject. The intelligent reader is, of course, aware that this question is purely a practical one, to be determined by experiment alone, and that all argument, theories, or hypotheses, are worse than useless—they are mischievous, tending to produce misconception.

It may be as well to add that the maximum speed of the vessel, with the 30½ revolutions of the engines per minute, was 10·2 knots per hour.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

[Report, October 27, 1863.]

From the London Mechanics' Magazine, Nov., 1863.

During the past month there have been examined 300 engines and 440 boilers. Of the latter, 32 have been examined specially, 7 internally, 31 thoroughly, and 370 externally, in addition to which 2 of these boilers have been tested by hydraulic pressure. The following defects have been found in the boilers examined:—Fracture 6 (1 dangerous); corrosion, 18 (2 dangerous); safety-valves out of order, 8; water-gauges ditto, 19 (4 dangerous); pressure-gauges ditto, 14 (1 dangerous); feed apparatus ditto, 5 (1 dangerous); blow-out apparatus ditto, 43; fusible plugs ditto, 4; furnaces out of shape, 5 (2 dangerous); over-pressure 3 (3 dangerous); blistered plates, 2. Total 127 (14 dangerous). Boilers without glass water-gauges 11; without pressure-gauges, 2; without blow-out apparatus, 13; without back-pressure valves, 43.

The three furnaces reported above as out of shape, all became so from overheating, consequent upon shortness of water, the injury in each case being observed for the first time, on setting to work in the morning, after the boiler had been standing during the night with the fires banked up, while two of the cases of injury were first observed on a Monday. Each of the boilers had but a single glass water-gauge, two of which were of the pillar construction, while all the boilers were fitted with fusible plugs, which proved inoperative in every instance. One of the boilers was fitted with a fusible plug in each furnace, both the crowns of which came down; the second was fitted with a fusible plug in the right-hand furnace only, and that was the one injured; while the third was fitted with a fusible plug in the left-hand fire-box, and the right-hand was the one injured; but in neither case did the fusible metal melt or give any sign.

There are several ways in which the water supply may run short in a boiler left standing for the night with the fire banked up, all of which should be guarded against.

Where there is only a single glass gauge, the attendant may be deceived as to the actual amount of water in the boiler; and thus the supply be left short overnight, unawares. It is well, therefore, to add a duplicate-gauge, so that one may act as a check upon the accuracy of the other; this would frequently prevent mistake.

Again, leakage may occur at the feed back-pressure valve, or at the blow-out tap, either from their being in bad condition or imperfectly closed. Engine-men are not sufficiently alive to the importance of examining these as a precaution, but leave them untouched until proved to be actually defective; whereas the feed back-pressure valve should be frequently taken out and its free action ascertained; while the blow-out tap also should be examined, the plug cleaned and greased, as well as ground up if necessary.

A considerable safeguard also to furnace crowns would be found in a low water safety valve, which lets off the pressure of the steam on a deficiency of water occurring within the boiler. It may be true that it would not necessarily prevent the furnace crowns from becoming overheated, since the fire would go on burning if left to itself, after the water supply had run short; but as the valve would not admit of any accumulation of steam, the injury would be confined to the plates over the fire, unless, by excessive carelessness, the attendant should let in the water and get up steam in the morning without observing the distorted furnace crown. Such, however, could not occur without the most culpable blindness, against which it is impossible to provide.

Explosions.—Five boilers not under the inspection of this Association, have exploded during the past month, from which eight persons have been killed and four others injured. Three of the boilers have been personally examined since the explosion.

The particulars of the first of these explosions may be briefly told. The boiler, which was not under the inspection of this Association, was the outer one of three working side by side and connected together, all of them being of the balloon or haystack class.

Each of these boilers was of large diameter, the one in question being about 18 feet at the base, the sides swelling to a still larger diameter at the springing of the hemispherical domed top. The bottom, which was arched, but had no stays to tie it to the crown, was worked into the sides by the plates being bent, and was seated upon a circular kerb of brick-work. Each of the boilers was fitted with a single safety-valve, of rather a rough description, while it was stated that the working pressure had not exceeded 5 ℔s., but that the exploded boiler, which was 20 years old, had already been repaired several times.

Immediately over the brick-work seating, the plates for some distance round the boiler had been seriously affected by external corrosion, and reduced in places to one-eighth of an inch in thickness. It was from this cause that the rent occurred, the rent running circumferentially for a distance of 10 or 12 feet parallel with the seating, and for the most part through the body of the plate. A portion of the bottom was blown down into the ash-pit where it remained, the rest of the shell flying to a distance of about 70 feet across a public road adjoining, and carrying away a portion of the boiler-shed in its flight. The engineer, as well as another man lying near to the boiler at the time, were both scalded, the latter dying shortly after in consequence.

Competent inspection could not have failed to detect the dilapidated condition of the boiler, while this explosion shows that, however moderate the pressure may be, no boiler can be safely worked unless frequently submitted to a searching examination.

The second, explosion from which one person was killed, arose from the collapse of the internal flue of a plain Cornish boiler which was not under the inspection of this Association.

The diameter of the shell was about 5 ft. 6 ins., that of the flue 3 ft., the length of both 28 ft., the thickness of the plates generally three-eighths of an inch, but in the flues barely as much, while the steam-pressure was 45 ℔s. per sq. inch. The collapse had extended from one end of the flue to the other, the portion over the fire being the least affected, while the remainder behind the bridge was completely flattened down. The boiler had not, however, stirred from its seat, or broken a steam-pipe joint, and all that was needed to resume work with the other two boilers, to which this was connected, was to close the junction valve. A rupture in an externally-fired boiler would have led to the dislocation of the whole series. The damage done by the last explosion of a boiler of that class was nearly £1000.

The cause of the explosion in question was obvious—viz: weakness of the flue. No flue of such dimensions as those just given can be safely worked with steam of a pressure of 45 ℔s. on the square inch, unless strengthened either with hoops or flanged seams, or stayed in some other suitable manner. It may be true, however, that some such flues, though unstayed, are working, and have done so for years with steam of an equal or even a greater pressure than the above; still they are continuing to do so only at a risk, and their past immunity from collapse is no security against its occurrence in the future. Some flues gradually work themselves out of the circular shape, and thus become considerably weakened; while all should be placed beyond the suspicion of danger, especially as this can be done at so trifling an expense. For particulars of the best method of adding hoops to those flues which are already made, see the Association's monthly report of June, 1862.

The fireman who was killed was standing in the front of the furnace at the time of the explosion, and blown, rake in hand, to a distance of about 60 feet.

The third explosion occurred to a Cornish boiler with a single flue, internally fired, and not under the inspection of this Association. Fortunately, no one was injured, the explosion occurring during the dinner hour when the men usually working near it were all absent.

The boiler was set on two side walls. It was 23 ft. long; its diameter in the shell was 6 ft.; and in the furnace 3 ft. 6 ins., which was reduced behind the fire bridge; while the thickness of the plates was seven-sixteenths of an inch.

The simple cause of the explosion was external corrosion, the plates having been so eaten away as to be reduced to one-sixteenth of an inch in thickness. Two complete belts, the entire circumference of the boiler, and a plate wide, had severed themselves from it; while a third, which had rent in a somewhat spiral direction, and considerably overlapped a single circumference, lay opened out, and still attached to the shell. The two detached belts had fallen, one on each side of the original position of the boiler, doing considerable damage to property thereby; while the main part of it had moved but slightly from its original seating, although it had made a semi-revolution on its longitudinal axis, and lay bottom upwards. The chimney was shattered; the boiler-house, as well as an adjoining building demolished, and some heavy machinery dismantled.

The Chemical Constitution of Pig Iron.

From the London Chemical News, No. 226.

Herr Rammelsberg has recently investigated the views of Karsten and Gurlt on the chemical constitution of pig iron (*see* Erdmann's *Jour. f. pr. Chem.* Bd. 89, S. 393) and has arrived at essentially different conclusions. According to Karsten, white and grey iron are chemical combinations of carbon with iron—the grey iron containing, at the same time, an admixture of graphite. This remains unchanged on the iron being dissolved in acids; while the chemically combined carbon—particularly with concentrated acids—is changed into a strongly odoriferous oil-like combination, the greater part of which volatilizes in the hydrogen, only a small portion dissolving in the acid. According to their views, Spiegeleisen is a chemical combination of iron with the maximum amount of carbon, 5 to 6 per cent., but containing no graphite.

Rammelsberg, on the contrary, has found 1·61 per cent. of graphite in the Müsen Spiegeleisen; and Bromeis, before him, found graphite in the white pig iron of Mägdesprung, as will be seen by the following analysis:—

	Carbon in combination Per cent.	Graphite Per cent.	Total. Per cent.
Bright white pig iron, .	2·518	0·500	3·018
Grey pig iron, . .	2·908	0·550	3·458
Spiegeleisen, . .	3·100	0·72	3·820

As, according to Rammelsberg, the Mädesprung Spiegeleisen only contains 3·9 per cent. of carbon, it seems that this class of iron may contain very unequal quantities of carbon, as well as an admixture of graphite, without any change in its character or properties. It might have been supposed that other metalloids replaced carbon—for example, silicon; but the analyses show, on the contrary, that the iron richest in carbon was also richest in silicon. In the Müsen Spiegeleisen there was found 1·5 per cent. of silicon, in that of Mädesprung 0·17, and in that of Styria from 0·01 to 0·27 per cent.—the latter only containing from 3·75 to 4·14 per cent. of carbon.

According to Gurlt, Spiegeleisen is a fourth carbide, and octahedric crystalline grey pig iron an eight carbide; the phosphorus, sulphur, and silicon being isomorphous with the carbon, and the manganese with the iron. But if in stating the atomic ratio between the electro-positive and the electro-negative constituents of the Spiegeleisen of Müsen and Mädesprung we adopt Rammelsberg's view, we have a ratio of 1 : 4·5 and 1 : 5·3 instead of 1 : 4. The grey octahedric crystals (the eighth carbide of Gurlt, approximating to wrought iron according to Tunner) are found to be very variable in composition, as shown by the following analysis :—

	From Rothehütte. By Rammelsberg.	From Lauchhammer. By Rammelsberg.	From Gleiwitz. By Gurlt.	From Lölling. By Richter.
Graphite, .	2·604	2·519	2·84	2·122
Carbon, .	0·201	0·373	2·46	0·967
Silicon, .	1·896	1·148	0·26	0·972
Phosphorus, .	0·065	0·406	?	0·021
Sulphur, .	0·069	0·043	?	0·003
Arsenic, .	——	——	——	0·005
Atomic ratio, .	1 : 19	1 : 21	1:8	12·5

Therefore, they are generally more than eight atoms of iron to one atom of carbon (silicon, phosphorus).

It will thus be seen that white as well as grey pig iron may crystallize under favorable circumstances, without the formation of the crystals being disturbed by the embedded graphite. The variation in the composition of the crystals, shown by the above analyses—taking place without any alteration in the crystalline form—can only be accounted for by the isomorphism of the elements; and according to Rammelsberg, such is the only possible explanation of the constitution of pig iron. As all the essential constituents of pig iron—such as iron, silicon, phosphorus, and carbon (as diamond)—crystallize on the regular system, and are consequently all isomorphous, we may consider pig iron as an isomorphous mixture of its constituents, which will explain the variation of its composition.

There are many examples of metals belonging to the regular system which form alloys that again crystallize on the same system; but there are also many alloys which crystallize on this system, whose constituent metals belong to distinct crystalline systems. On the other hand, metals that crystallize on the regular system form alloys crystallizing on a different system; thus, for example, silver, zinc, nickel, and copper (iron), alloyed with antimony, crystallize on the pyramidal system. If, therefore, Spiegeleisen is pyramidal, as is probably the case, it belongs to this class of isomorphous mixtures ; and the difference of form in grey and white pig iron results from the heteromorphism of their isomorphous constituents. This heteromorphism is evidently a general characteristic of the elements, being just as marked in the metals as is in sulphur and carbon. Doubtless the rhombohedric metals—Sb, As, Fe, Bi, Zn, Pd, Ir, and Rd,—and the tetragonal Sn, isomorphous to Bo, may, under certain circumstances, crystallize on the regular system, while Au, Ag, Cu, Pb, &c., and also Sn, may be rhombohedric.—*Mining Journal.*

On Boiling Water. By W. Grove, Esq., Q.C., F.R.S., M.R.I.

From the London Chemical News, No. 225.

A paper by M. Donny ("*Mémoires de l'Académie Royale de Bruxelles*," 1843,) makes known the fact that in proportion as water is deprived of air, the character of its ebullition changes, becoming more and more abrupt, and boiling like sulphuric acid with *soubresauts*, and that between each burst of vapor the water reaches a temperature above its boiling point. To effect this, it is necessary that the water be boiled in a tube with a narrow orifice, through which the vapor issues; if it be boiled in an open vessel it continually re-absorbs air and boils in the ordinary way.

In my experiments on the decomposition of water by heat, I found that with the oxyhydrogen gas given off from ignited platinum plunged into water, there was always a greater or less quantity of nitrogen mixed; this I could never entirely get rid of, and was thus led into a more careful examination of the phenomenon of boiling water, and set before myself this problem—what will be the effect of heat on water, perfectly deprived of air or gas?

Two copper wires were placed parallel to each other through the neck of a Florence cask, so as nearly to touch the bottom; joining the lower ends of these was a fine platinum wire, about an inch and a-half long, and bent horizontally into a curve. Distilled water, which had been well boiled and cooled under the receiver of an air-pump, was poured into this flask so as to fill about one-fourth of its capacity. It was then placed under the receiver of an air pump, and one of the copper wires brought in contact with a metallic plate covering the receiver, the other bent backwards over the neck of the flask and its end made to rest on the pump-plate. By this means, when the terminal wires from a voltaic battery were made to touch the one, the upper and the other lower plate, the platinum wire would be heated, and the boiling continued indefinitely in the vacuum of a very excellent air-pump. The effect was very curious: the water did not boil in the ordinary manner, but at intervals a burst of vapor took place, dashing the water against the sides of the flask, some escaping into the receiver. (There was a projection at the central orifice of the pump-plate to prevent this overflow getting into the exhaustion tube.)

After each sudden burst of vapor, the water became perfectly tranquil, without a symptom of ebullition until the next burst took place. These sudden bursts occurred at measured intervals, so nearly equal in time that, had it not been for the escape from the flask at each burst of a certain portion of water, the apparatus might have served as a time-piece.

This experiment, though instructive, did not definitely answer the question I had proposed, as I could not, of course, ascertain whether their was some minute residuum of gas which would form the nucleus of each ebullition; and I proceeded with others. A tube of glass, 5 ft. long and $\frac{4}{10}$ths inch internal diameter, was bent into a V-shape; into one end a loop of platinum wire was hermetically sealed with great care, and the portion of it in the interior of the tube was platinized. When the tube had been washed, distilled water which had been well purged of air as before, was poured into it to the depth of 8 inches, and the rest of the tube filled with olive oil; when the V was inverted, the open end of the tube was placed in a vessel of olive oil, so that there would be 8 inches of water resting on the platinum wire, separated from the external air by a column of 4 ft. 4 ins. of oil. The projecting extremities of the platinum wire were now connected with the terminals of a voltaic battery, and the water heated; some air was freed and ascended to the level of the tube—this was made to escape by carefully inverting the tube so as not to let the oil mix with the water—and the experiment continued. After a certain time the boiling assumed a uniform character, not by such sudden bursts as in the Florence flask experiment, but with larger and more distinct bursts of ebullition than in its first boiling.

The object of platinizing the wire was to present more points for the ebullition, and to prevent *soubresauts* as much as possible.

The experiment was continued for many hours, and in some repetitions of it for days. After the boiling had assumed a uniform character, the progress of the vapor was carefully watched, and as each burst of vapor condensed in the oil, which was kept cool, it left a minute bead of gas, ascended through the oil to the bend of the tube: a bubble was formed here which did not seem at all absorbed by the oil. This was analyzed by a eudiometer, which I will presently describe, and proved to be nitrogen. The beads of gas, when viewed through a lens and micrometer scale at the same height in the tube, appeared as nearly as may be of the same size. No bubble of vapor was condensed completely, or without leaving this residual bubble. The experiment was frequently repeated, and continued until the water was so nearly boiled away that the oil, when disturbed by the boiling, nearly touched the platinum wire; here it was necessarily stopped.

To avoid any question about the boiling being by electrical means, similar experiments were made with a tube, without a platinum wire, closed at its extremity, and the boiling was produced by a spirit lamp. The effects were the same, but the experiment was more difficult and imperfect, as the bursts of vapor were more sudden, and the duration of the intervals more irregular.

The beads of gas were extremely minute, just visible to the naked eye, but were made visible to the audience by means of the electric lamp.

In these experiments there was no pure boiling of water—*i.e.*, no rupture of cohesion of the molecules of water itself, but the water was boiled, to use M. Donny's expression, by evaporation against a surface of gas.

It is hardly conceivable that air could penetrate through such a column of oil, the more so as the oil did not perceptibly absorb the nitrogen freed by the boiling water and resting in the bend of the tube; but to meet this conjectural difficulty, the following experiment was made: —A tube, 1 foot long and $\frac{2}{10}$ths inch internal diameter, bent into a slight angle, had a bulb $\frac{3}{4}$-inch diameter blown on it at the angle; this angle was about 3 inches from one end and 9 from the other; a loop of platinum wire was sealed into the shorter leg and the whole tube and bulb filled with and immersed into mercury, water distilled, and purged of air as before was allowed to fill the short leg, and by carefully adjusting the inclination, the water could be boiled so as to allow bubbles to ascend into the bulb and displace the mercury. The effect was the same as with the oil experiment,—no ebullition, without leaving a bead of gas; the gas collected in the bulb, and was cut off by what may be termed a valve of mercury, from the boiling water, then allowed to escape, and so on; the experiment was continued for many days and the bubbles analyzed from time to time; they proved, as before, to be nitrogen; and, as before, continued indefinitely.

A similar experiment was made without the platinum wire, and though, from the greater difficulties, the experiment was not so satisfactory, the result was the same.

As the mercury of the common barometer will keep air out of its vacuum for years, if not for centuries, there could be no absorption here from the external atmosphere, and I think I am fairly entitled to conclude from the above experiments—which I believe went far beyond any that have been recorded—that no one has yet seen the phenomenon of pure water boiling—*i.e.*, of the disruption of the liquid particles of the oxyhydrogen compound so as to produce vapor which will, when condensed, become water, leaving no permanent gas. Possibly, in my experiment of the decomposition of water by ignited platinum, it may be that the sudden application of intense heat, and in some quantity, so forces asunder the molecules, that not having sufficient nitrogen dissolved to supply them with a nucleus for evaporation, the intregal molecules are severed, and the decomposition takes place. If this be so, and it seems to me by no means a far-fetched theory, there is probably no such thing as boiling, properly so called, and the effect of heat on liquid in which there is no dissolved gas may be to decompose them.

Considerations such as these led me to try the effect of boiling on an elementary liquid, and bromine occurred as the most promising one to work upon; as bromine could not be boiled in contact with water, oil, or mercury, the following plan was ultimately devised:—A tube, 4 feet long and $\frac{4}{10}$ths inch diameter, had a platinum loop sealed into one closed extremity; bromine was poured into the tube to the height of 4 inches; the open end of the tube was then drawn out to a fine point by the blow-pipe, leaving a small orifice; the bromine was then heated by a spirit-lamp and when all the air was expelled, and a jet of bromine vapor issued from the point of the tube, it was sealed by the blow-pipe. There was then, when the bromine vapor had condensed a vacuum in the tube above the bromine. The platinum loop was now heated by a voltaic battery, and the bromine boiled: this was continued for some time, care being taken that the boiling should not be too violent. At the end of a certain period—from half-an-hour to an hour—the platinum loop gave way, being corroded by the bromine; the quantity of this had slightly decreased. On breaking off, under water, the point of the tube, the water mounted, and showed a notable quantity of permanent gas, which on analysis proved to be pure oxygen. As much as a quarter of a cubic inch was collected at one experiment. The platinum wire which had severed at the middle, was covered with a slight black crust, which, suspecting to be carbon, I ignited by a voltaic spark in oxygen in a small tube over lime-water; it seemed to give a slight opalescence to the liquid, but the quantity was so small that the experiment was not relied on. No definite change was perceptible in the bromine; it seemed to be a little darker in color and had a few black specks floating in it, which I judged to be minute portions of the same crust which had formed on the platinum wire, and which had become detached.

The experiment was repeated with chloride of iodine, and with the same result, except that the quantity of oxygen was greater: I collected as much as half a cubic inch in some experiments, from an equal quantity of chloride of iodine; the platinum wire, however, was more quickly acted on than with bromine, and the glass of the tube around it to some extent.

Melted phosphorus was exposed to the heat of the voltaic disruptive discharge by taking this between platinum points in a tube of phosphorus, similarly to an experiment of Davy's, but with better means of experimenting; a considerable quantity of phosphuretted hydrogen was given off, amounting in several experiments to more than a cubic inch.

A similar experiment was made with melted sulphur, and sulphuretted hydrogen was given off, but not in such quantities as the phosphuretted hydrogen. I tried in vain to carry on these experiments beyond a certain point; the substance became pasty, mixed with platinum from the arc, and from the difficulty of working with the same freedom as when they were fresh, the glass tubes were always broken after a certain time. Had I time for working on the subject now, I should use the discharge from the Ruhmkorf coil which had not been invented at the period of these experiments. At a subsequent period when this discharge was taken in the vacuous receiver of an air-pump from a metallic point to a metallic capsule containing phosphorus, a considerable yellow deposit lined the receiver, which, on testing, turned out to be allotropic phosphorus. No gas is however given off. I had an air-pump (described "Phil. Trans.," 1852, p. 101) which enabled me to detect very small quantities of gas, but I could get none. It was in making these experiments that I first detected the striæ in the electric discharge, which have since become a subject of such interesting observations, which are seen, perhaps, more beautifully in this phosphorus vapor than in any other medium, and which cease or become very feeble, where the allotropic phosphorus is not produced.

I tried also phosphorus highly heated by a burning-glass in an atmosphere of nitrogen, but could eliminate no perceptible quantity of gas, though the phosphorus was changed into the allotropic form.

It is not difficult to understand why gas is not perceptibly eliminated in the last two experiments; the effect is probably similar to that described in my paper on the "*Decomposition of Water by Heat*," where when the arc or electric spark is taken in aqueous vapor, a minute bubble of oxyhydrogen gas is freed and disseminated through the vapor, re-combination being probably prevented by this dilution; but, however long the experiment may be continued, no increased quantity of the gas is obtained, all beyond this minute quantity being re-combined. If, however, the bubble of gas be collected, by allowing the vapor to cool, and then expelled, a fresh portion is decomposed, and so on.

So with the phosphorus in the experiments in the air-pump and with the burning-glass; if any gas is liberated it is probably immediately re-combined with the phosphorus; possibly a minute residuum might escape re-combination, but the circumstances of the experiment did not admit of this being collected, as the gas was with the aqueous vapor.

When, on the other hand, the gas freed is immediately cut off from the source of heat, as when the spark is taken in liquids, an indefinite quantity can be obtained.

Decomposition and the elimination of gas may thus take place by the application of intense heat to a point in a liquid, or also in gas or vapors; but in the latter case it is more likely to be masked by the quantity of gas or vapor through which it is disseminated.

I believe there are very few gases in which some alteration does not take place by the application of the intense heat of the voltaic arc or electric spark. If the arc be taken between the platinum points in dry oxygen gas over mercury, the gas diminishes indefinitely, until the mercury rises, and by reaching the point where the arc takes place, puts an end to the experiment. I have caused as much as a cubic inch of oxygen to disappear by this means. I at one time thought this was due to the oxidation of the platinum; but the high heat renders this improbable, and the deposit formed on the interior of the glass tube in which the experiment is made has all the properties of platinum-black; so if the spark from a Ruhmkorf coil be taken in the vapor of water for several days, a portion of gas is freed which is pure hydrogen, the oxygen freed being probably changed into ozone, and dissolved by the water in this case, while in the former it combined with the mercury.

I have alluded to the eudiometer by which I analyzed the gases obtained in these experiments; it was formed simply of a tube of glass, frequently not above 2½ millimetres in diameter, with a loop of wire hermetically sealed into one end, the other having an open bell mouth. By a platinum wire a small bubble of the gas to be examined could be got up through water or mercury into the closed end of the tube, and by the addition of a bubble of oxygen or hydrogen gas, a very accurate analysis of very minute quantities of gas could be made: I have analyzed by this means quantities no larger than a partridge-shot.

I need hardly allude to results on the compound liquids, such as oils and hydrocarbons, as the fact that permanent gas is given off in boiling such liquids would not be unexpected; but the above experiments seem to show that boiling is by no means necessarily the phenomenon that has generally been supposed, viz: a separation of cohesion in the molecules of a liquid from distention by heat. I believe, from the close investigation I made into the subject, that (except with the metals, on which there is no evidence) no one has seen the phenomenon of pure boiling without permanent gas being freed, and that what is ordinarily termed boiling arises from the extrication of a bubble of permanent gas either by chemical decomposition of the liquid, or by the separation of some gas associated in minute quantity with the liquid, and from which human means have hitherto failed to purge it; this bubble once extricated, the vapor of the liquid expands it, or, to use the appropriate phrase of M. Donny, the liquid evaporates against the surface of the gas.

My experiments are, in a certain sense, the complement of his. He showed that the temperature of the boiling point was raised in some proportion as water was deprived of air and that under such circumstances the boiling took place by *soubresauts*. I have, I trust, shown that when the vapor liberated by boiling is allowed to condense, it does not altogether collapse into a liquid, but leaves a residual bubble of permanent gas, and that at a certain point this evolution becomes uniform.

Boiling, then, is not the result of merely raising a liquid to a given temperature; it is something much more complex.

One might suppose that with a compound liquid the initial bubble by which evaporation is enabled to take place, might, if all foreign gas were or could be extracted, be formed by decomposition of the liquid: but this could not be the case with an elementary liquid; whence, the oxygen from bromine or the hydrogen from phosphorus and sulphur?

As with the nitrogen in water, it may be that a minute portion of oxygen, hydrogen, or of water, is inseparable from these substances, and that if boiled away to absolute dryness, a minute portion of gas would be left for each ebullition.

With water there seems a point at which the temperature of ebullition and the quantity of nitrogen yielded become uniform, though the latter is excessively minute.

The circumstances of the experiments with bromine, phosphorus, and sulphur did not permit me to push the experiment so far as was done with water; but as far as it went the result was similar.

When an intense heat, such as that from the electric spark or voltaic arc is applied to permanent gas, there are, in the greater number of cases, signs either of chemical decomposition or of molecular change; thus compound gases, such as hydrocarbons, ammonia, the oxides of nitrogen, and many others are decomposed. Phosphorus in vapor is changed to allotropic phosphorus, oxygen to ozone, which, according to present experience, may be viewed as allotropic oxygen. There may be many cases where, as with aqueous vapor, a small portion only is decomposed, and this may be so masked by the volume of undecomposed gas as to escape detection. If, for instance, the vapor of water were uncondensible, the fact that a portion of it is decomposed by the electric spark or ignited platinum would not have been observed.

All these facts show that the effect of intense heat applied to liquids and gases is much less simple, and presents greater interest to the chemist, than has generally been supposed. In far the greater number of cases, possibly in all, it is not mere expansion into vapor which is produced by intense heat, but there is a chemical or molecular change. Had circumstances permitted, I should have carried these experiments further, and endeavored to find *experimentum crucis* on the subject. There are difficulties with such substances as bromine, phosphorus. &c., arising from their action on the substances used to contain and heat them, which are not easy to vanquish, and those who may feel inclined to repeat my experiments will find these difficulties greater than they appear in narration ; but I do not think they are insuperable, and hope that, in the hands of those who are fortunate enough to have time at their disposal, they may be overcome.

To completely isolate a substance from the surrounding air, and yet be able to experiment on it, is far more difficult than is generally supposed. The air-pump is but a rude mode for such experiments as are here detailed.

Caoutchouc joints are out of the question. Even platinum wires carefully sealed into glass, though, as far as I have been able to observe, forming a joint which will not allow gas to pass, yet it is one through which liquids will effect a passage, at all events, when the wires are repeatedly heated.

In some experiments with the ignited platinum wire hermetically sealed into a tube of glass, the end of the tube containing the platinum wire was placed in a larger tube of oil, to lessen the risk of cracking the glass. After some days experimenting, though the sealing remained perfect, a slight portion of carbon was found in the interior liquid. This does not affect the result of my experiments, as I repeated them with glass tubes closed at the end and platinum wires, and also without the oil-bath: but it shows how difficult it is to exclude sources of error. When water has been deprived of air to the greatest practicable extent it becomes very avid for air. The following experiment is an instance of this:—A single pair of the gas battery, the liquid in which was cut off from the external air by a greased glass stopper, having one tube filled with water, the other with hydrogen ; the platinized platinum plates in each of these tubes were connected with a galvanometer, and a deflection took place from the reaction of the hydrogen on the air dissolved in the water. After a time the deflection abated, and the needle returned to zero, all the oxygen of the air having become combined with the hydrogen. If now the stopper were taken out, a deflection of the galvanometric needle immediately took place, showing that the air rapidly enters the water as water would a sponge. Absolute chemical purity in the ingredients is a matter, for refined experiments, almost unattainable. The more delicate the test, the more some minute residual product is detected. It would seem (to put the proposition in a somewhat exaggerated form) that in nature everything is to be found in anything if we carefully look for it.

I have indicated the above sources of error to show the close pursuit that is necessary when looking for these minute residual phenomena. Enough has, I trust, been shown in the above experiments to lead to the conclusion that hitherto simple boiling, in the sense of a liquid being expanded by heat into its vapor, without being decomposed or having permanent gas eliminated from it, is a thing unknown. Whether such boiling *can* take place may be regarded as an open question though I incline to think it cannot; that if water, for instance, could be absolutely deprived of nitrogen, it would not boil until some portion of it was decomposed; that the physical severance of the molecules by heat is also a chemical severance. If there be anything in this theoretic view, there is great promise of important results on elementary liquids, if the difficulties to which I have alluded can be got over.

The constant appearance of nitrogen in water, when boiled off out of contact with the air almost to the last drop, is a matter well worthy of investigation. I will not speculate on what possible chemical connexion there may be between air and water. The preponderance of these two substances on the surface of our planet, and the probability that nitrogen is not the inert diluent in respiration that is generally supposed, might give rise to not irrational conjectures on some unknown bond between air and water. But it would be rash to announce any theory on such a subject; better to test any guess one may make by experiment than to mislead by theory without sufficient data, or to lessen the value of facts by connecting them with erroneous hypotheses.

Preservation of Meat.

From the Journal of the Society of Arts, No. 590.

The preservation of meat, whether for the use of our sailors on board ship, or for other purposes, has long engaged the attention of scientific and practical men, and various ways of affecting this object have been from time to time devised. The methods hitherto adopted on a large scale have been the packing of cooked meat in air-tight cases, or impregnating it with salt, and keeping it in barrels immersed in brine. The first, though effectual for preserving the meat for almost any amount of time, leaves the flesh, even when the utmost care is taken in the process, more or less insipid and tasteless; the second, though also preservative for a considerable time, renders the meat not only flavorless, but absolutely extracts from it, as Liebig tells us, nearly all its nutritive properties, as well as those peculiar qualities which are necessary for keeping the body in health. It is well-known that a long continuance of such food, thus prepared, engenders scurvy. The Admiralty are now making experiments with a process devised by Dr. Morgan, an Irish gentleman; and a few weeks since some animals were slaughtered, and their carcases subjected to this process in the presence of officers of her Majesty's Victualling Department at Deptford. A bullock having been killed in the usual way, the chest was immediately opened, and a metal pipe with a stop-cock inserted in connexion with the arterial system. This pipe was connected by means of elastic tubing, with a tub filled with brine placed at an elevation of about twenty feet above the floor. The stop-cock being turned, the brine forced itself through the arteries of the animal, and, passing through the capillaries, flowed back through the veins, carrying with it all the blood, making its exit by means of an incision provided for that purpose. About six gallons of brine passed thus through the body, washing out blood from the vessels. Having thus cleared all the vessels, the metal pipe was connected with another tube similarly placed, containing the preservative material to be injected, and at the same time their exit, after traversing the body, was prevented. On communication being made, the liquid became forced into the vessels, and, by means of the pressure, it penetrated into every part of the animal, and might be seen exuding at any point where an incision was made. The liquid used on the occasion of the late experiments consisted of six gallons and a half of brine, 10 lbs. of sugar, $\frac{3}{4}$-℔. of saltpetre, with half a bottle of catchup, and an infusion of cloves and pepper. The whole process is very rapid, and is extremely simple, requiring nothing that can be termed machinery. It took no longer than three minutes to send the first six gallons of liquid through the animal to wash out the vessels, and about three minutes more to inject the animal with the preservative liquid. Indeed, so rapid is the whole proceeding, that, even on the occasion above referred to, where the men were unaccustomed to the work, and the arrangements were necessarily imperfect, the time occupied was only twenty minutes from the killing the animal to the complete infiltration being made. The beast is then skinned, cut up into pieces, large or small, as may be required, and taken to a drying room, where it is hung until thoroughly dried, after which it is packed in boxes with sawdust and charcoal. It is confidently believed that the meat treated in this manner will stand any climate, and the flesh is free from the insipidity of that ordinarily preserved in tins, and its goodness is not destroyed by having nearly all its valuable properties drawn out by immersion in brine. So far as its preservative powers have been tested in this country, the process is stated to answer the purpose. A purveyor at Portsmouth has for some time past treated meat in this way with success, and sells it in the regular course of trade. It is obvious that any variety of liquids, chemicals, or condiments may be thus injected into the animal, and the meat flavored in any way that may be thought desirable; the meat may also be dried or cured like ham or bacon, if so wished. Indeed, it would seem that the method is peculiarly fitted for this purpose. In hot countries, and in countries where animals are abundant, and where now they are bred almost entirely for their wool, fat, and hides, the process seems especially valuable, as by it the meat, instead of being thrown away, might be rendered available for export for food to other countries. The Victualling Department have had a few animals thus prepared for experiment, and it is intended to send the meat out on voyages to various parts of the world to test its keeping qualities. So little machinery is required, that a ship's crew could readily carry out the process at any place where they could land and animals were abundant, and thus lay in a store of meat which, although, no doubt, salted to a certain extent, would not have the same disadvantages in a sanitary point of view as meat preserved in brine-pickle. Some lengths of india-rubber tubing, pieces of metal tube with stop-cocks, and tubs for holding the liquid, are all that is required. In hot climates the drying may be effected in the open air, and in other cases there would be no difficulty in arranging a room for the purpose, either on shore or on board ship.

Description of a New Machine for drawing Solid Metal Tubing.

From the Journal of the Society of Arts, No. 589.

A new machine for drawing solid metal tubes has lately been tested in London with considerable success. The system was originally introduced into England by a French gentleman, about ten years ago, but in consequence of defective mechanical arrangements was at that time unsuccessful. The Stephenson Tube Company have lately erected extensive works at Birmingham for the manufacture of brass and copper tubes on this principle, but the new machine is especially adapted to the formation of tubes of steel, and other hard and close-grained metals. The ordinary method of forming wrought iron tubes is by bending round a long narrow plate of the metal so that the edges meet, and then having reduced them to a welding heat, to join them together by drawing the tube through a die made for that purpose. The machine in question entirely differs from this, and its action may be described as follows:—It consists of two large cast iron cylinders, 11 feet long, placed opposite each other, and connected by a ram of 10 feet travel, which is driven out of the one and into the other alternately. On each end of the cylinders is a massive flange, pierced with eight holes, some four inches across; on the ram is a similar flange, but double, offering eight holes to each cylinder. The diameter of this hydraulic ram is 16½ inches, and the force obtainable upwards of 600 tons. A draw-plate or die of peculiar form is placed in one of the holes in an inner flange of one of the cylinders, and the tube to be drawn slightly tapered at the end, to allow of its passing into the die, is placed therein, having previously received a steel-headed mandril, intended to act on the interior of the tube. The tube is fixed on the flange of the ram by means of a screw pin projecting through the flange of the cylinder, and the stem of the mandril is fastened to the further end of the cylinder. The water being forced into the cylinder the ram is driven forward, and the tube is drawn bodily through the die, or draw-plate, and over the mandril, the head of which, inside the tube, is placed within the circle of contact of the die outside. By this means the tube is drawn externally and internally at the same time, and an eighth of an inch of steel may, at a time, be displaced from the surface, the tube being elongated proportionately; but in practice it is found more convenient to take off less at each pass in order to avoid fatiguing the metal overmuch. It is stated that two bars of rough iron, as delivered from the forge, 3½ inches diameter and 4 feet long, were placed in the machine and drawn through the die, the drawing occupying some five minutes. The bars issued from the die with a surface finer than can be obtained at present by any known mechanical means, except continued friction, and infinitely truer than any turning-lathe produces. It would take a man at a lathe two days at least to turn these bars, and he would then produce inferior results to those obtained in perhaps a quarter of an hour, if the time employed to fix and unfix the bars in the hydraulic drawing bench be included. It will scarcely be necessary to remark, in conclusion, that should this machine fulfil the expectations of the inventor, it will cause a complete revolution in the manufacture of gun-barrels, hollow shafting, axles, piston-rods, and such-like articles.

*To make an Absolutely Correct Camera copy of a Chart by Means of a Single Distorting Lens.** By Robert H. Bow, C. E.

From the Lond. Civ. Eng. and Arch. Jour., December, 1863.

I have already mentioned before this society that distortion in a negative may be corrected in the positive taken from it by the camera, whether the distortion arises from the defects of the lens, or from the camera not having been placed squarely to the object in taking the negative. This short paper is merely to call attention to a very simple practical application of the method of correcting that distortion which arises from the defects of the lens.

I have here a camera with an achromatic plano-convex lens of 4½ ins. focal length. It produces pictures which, if of moderate size, are free from anything that a casual observer would detect as distortion; but the following table will give you an idea of the real amount thereof. Before you I have laid a negative taken by the same lens, embracing a field of 80°.

Table showing the Distortion of Scale (measured in the direction of a diameter of the picture) when a distant† view is taken with the plano-convex achromatic lens of four and a half inches focal length, described at page 241 of the current volume of *The Civil Engineer and Architect's Journal*, the stop being three-quarters of an inch from the flat side. (The points in the image corresponding with the pencils at the several angles of obliquity, are obtained by the method referred to in my paper on the "Curvatures of the Image.")

	Intervals measured (approximately) from the actual image.	Intervals according to calculation when there is no distortion.
Between rays at 0° and 5°	100	100·00
" " 5° " 10°	101	101·54
" " 10° " 15°	102	104·73
" " 15° " 20°	103	109·75
" " 20° " 25°	105	116·97
" " 25° " 30°	107	126·93
" " 30° " 35°	110	140·42

From this table it appears, then, that an object lying between 30° and 35° of obliquity will suffer a contraction from 140·42 to 110, or a diminution of 30·42 upon a length of 140·42, which is equivalent to

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* Read at a meeting of the Edinburgh Photographic Society, 21st October, 1863.

† When the object is not so distant, the distortion will be somewhat greater.

21·7 per cent. as compared with the size it would appear if seen lying within the 5° of obliquity. Of course this is an extreme obliquity, but it is the accompanying distortion which chiefly interferes with our using such an angle, as it may be observed that the negative exhibited is sufficiently sharp for many purposes up to that extent, or for a field embracing 70°.

Between the obliquities of 20° and 25° the distortion is equivalent to a contraction of 117 into 105, or a reduction of 12 on a length of 117, which amounts to a diminution of 10·3 per cent. compared with the scale at the central intervals. Now even this will be allowed by all to be serious; for it is as though a foot measure, given correctly near the centre, measured only 10·8 inches near the side of the picture.

Let us now proceed to explain the method of copying without any distortion, whatever the field embraced:—

Fig. 1.

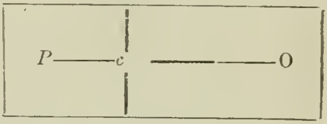


Fig. 2.

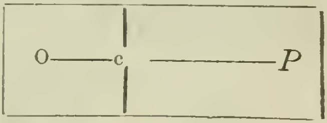
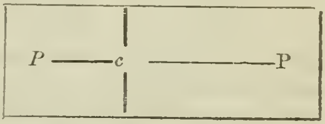


Fig. 3.



In the figures let O represent the object or chart, *P* the photograph, and *c* the lens—the line beside *c* indicating the position of the stop.

In Fig. 1, *P* represents the negative first obtained with the stop, arranged as usual for views, and *P* will therefore be "barrelled." In Fig. 2, if the chart were placed at O, we should get a distorted negative *P* of the "pincushion" order. But if instead of a straight object O, in Fig. 2, we place the barrelled negative *P*, as in Fig. 3, we get a copy at P, free from all distortion, if made of the same size as O, since the tendency of the arrangement to produce pincushioning then just suffices to correct the barrelling. In fact each pencil of light is sent back from the negative *P*, in Fig. 3, to the final copy *P*, in exactly the same course as that by which it arrived in the first instance from O, in Fig. 1 (except in so far as the focussing may be defective.)

This absolutely perfect correction of the distortion only takes place when the final copy is made of the same size with the original chart. But practically, if no great difference exists between the lengths of *P c* in Figs. 1 and 3, the correction may be assumed as sufficiently perfect; and in order that these distances may not vary much, we should make our negative considerably less than the final copy to be made from it, when the final copy is to differ much in scale from the chart.

If the negative be made of intermediate size between the chart and the final copy, and the latter be taken with the stop on its side of the lens, the correction of the barrelling will be overdone, so that the final copy will be slightly pincushioned, but generally so slightly that this mode of proceeding will frequently be preferred when a very sharp result is desired.

If it be desired that the final photograph P, in Fig. 3, should be a negative, we may effect this by substituting for P a transparency printed from the first negative by superposition.

Coal and Iron in France and other Countries.

From the Journal of the Society of Arts, No. 590.

A work on property in minerals, by M. Edouard Dalloz, was presented a few days since to the Academy of Moral and Political Sciences of Paris, by M. Wolowski, who read a report on the book in question and on various matters connected with the subject on which it treated. M. Dalloz treats especially of mineral legislation in France and Belgium, but touches incidentally on that of other nations, and also upon the statistics and economy of the mineral question in general, its bearings and influences. In connexion with the statistical portion of the question, M. Wolowski was able to avail himself of more recent records than those used by M. Dalloz, and made an elaborate report to the Academy, from which the following is extracted:—In 1789 France used only half a million tons of coal a year, and of this little more than one-half was raised at home; but in 1830 the production had grown to 1,800,000, and the quantity imported to 600,000 tons. The progress made since that time is as follows:—

	Production. Tons.	Importation. Tons.	Consumption. Tons.
1835,	2,500,000	800,000	3,300,000
1840,	3,000,000	1,290,000	4,290,000
1845,	4,000,000	2,200,000	6,200,000
1847,	5,400,000	2,500,000	7,600,000

During the four following years political events stopped the growth, both of production and consumption; the quantity raised in France fell back to four millions of tons, and the imports to little more than two millions. Since 1852 the progress was great and constant—

1853,	5,900,000	3,500,000	9,400,000
1854,	6,000,000	4,000,000	10,000,000
1855,	7,400,000	5,000,000	12,400,000
1856,	7,900,000	5,000,000	12,900,000
1857,	7,900,000	5,195,000	13,095,000
1858,	7,353,000	4,840,000	12,200,000
1859,	7,483,000	4,900,000	12,400,000

In 1860 came the change in the tariff, and the following are the results to the present time:—

1860,	8,391,000	5,200,000	13,600,000
1861,	8,400,000	5,300,000	13,700,000
1862,	9,400,000	5,200,000	14,600,000
1863,	10,000,000	5,200,000	more than 15,000,000

The return for last year being an approximate estimate made by the Imperial Administration of Mines. By these figures it will be seen that France raises for herself at present as much as she consumed in the whole of 1854; and that the consumption itself has doubled since 1847. It will be observed also, a fact not noted in the report, that since 1860 there has scarcely been any increase in the imports, while the home production has grown about 20 per cent. Side by side with these facts M. Wolowski drew special attention to the fact that the quantity of coal brought to the surface in England had grown from twenty-six millions of tons in 1837, to eighty-four millions in 1862, and that she produced far more than the half of all the coal raised in the world. The produce of America was given at fifteen, that of Belgium at nine, and of Germany at upwards of fourteen millions of tons per annum. As regards iron, the whole of the furnaces in France only produced 69,000 tons of pig iron in 1789, 115,000 tons in 1812, and 112,500 tons in 1819. Since that time there has been a great increase, and it is important, says M. Wolowski, to see what has been the effect of the commercial treaty of 1860. In 1830 the quantity of pig iron had risen to 347,773 tons, and that of wrought iron to 237,379 tons, and in 1847 it had grown to 602,772 tons of the former, 376,686 of the latter. In 1848 the totals had fallen off to the extent of 50 per cent. No reaction occurred till 1854; but in 1859 the totals were 856,000 and 520,000 respectively, rather less than those of 1858, and considerably below those of the previous year. During the years 1860-61 a great increase took place, and in 1862 the amount of pig iron produced rose to 1,053,000 tons, while that of 1863 is estimated at 1,180,000 tons. Of the last total, says M. Wolowski, 280,000 tons were charcoal iron, the remaining 900,000 tons having been produced with the aid of coal alone or mixed. As regards wrought iron the report gives the following return for the year 1863:—

Charcoal iron, ...	76,800 Tons.
Coal iron, ...	606,000 "
Mixed fuel, ...	22,700 "
Total, ...	705,000 "

The production of charcoal iron has fallen in the last three years from 90,655 to 76,800 tons, while that of coal iron has risen from four to six hundred thousand tons. In 1847, more than half the iron produced in France was charcoal iron. The total product of all Europe in 1808, according to M. Héron de Villefosse, was only 825,000 tons of pig iron, while the quantity now made is about seven millions of tons, of which England produces nearly four; and in 1862 France 1,180,000; Germany, 591,593; Sweden, 143,000; Austria 270,000; and Belgium, 161,000 tons. After such totals as these it is curious to note that in 1740 England made none but charcoal iron, and only produced 17,500 tons in the year.

Description of a New Gun invented by James Mackay.

From the Lond. Mechanics' Magazine, April, 1864.

Two or three weeks ago the world heard for the first time of the Mackay gun. Yet, short as this space is, it has been quite long enough to demonstrate that Mr. Whitworth and Sir W. Armstrong must indeed look to their laurels. Mr. James Mackay is a Liverpool timber merchant, and the gun which will in future be known by his name has, we believe, been constructed wholly at his own expense, certainly without Government aid. The principles involved in its construction are novel, elegant, and ingenious. Hitherto we have learned to regard windage as a thing to be avoided by the use of every expedient at our command, because it wastes power and leads to inaccuracy of flight in the projectile. Mr. Mackay, on the other hand, turns this same windage to account, and makes it the motive power for spinning his projectiles round. With a smooth bore, it is evident that the gas generated would make its escape quicker than the heavy shot. To prevent this, spiral grooves of much greater acuteness than those employed in the ordinary rifled cannon run through the interior of the Mackay gun, and as the gas follows the course of these grooves, it has to travel about six feet further than the shot before the muzzle is reached. It is thus prevented from escaping before the shot, which is whirled swiftly round by the revolving current of gas that follows close upon and grasps it, as it were, in its embrace, during its passage through the chase of the gun. The rotary motion is thus imparted to the shot solely by the friction of the escaping gases on its smooth sides, the gun having in all respects, save these spiral grooves—which must not be confounded with rifling proper—the character of a smooth bore, as the shot rests, upon the raised surfaces of the grooves, by which its motion is not impeded. Thus the initial velocity of the smooth bore is obtained, as well as the rotary motion necessary to precision; and considering the importance of high velocities as exemplified in the effectiveness of the old 68-pounder at short distances, it is a great deal in favor of the Mackay gun that it discharges its shot with a momentum of some 300 or 400 feet more per second than does the Armstrong gun. The projectiles used are cylindrical steel bolts with a conical head. They are balanced from the centre, and the smaller weight at the cone end is compensated for by scooping out a portion of the metal at the other end, and they are perfectly smooth.

The experimental gun has been manufactured of wrought iron by the Mersey Steel and Iron Works Company. It weighs 9 tons, has a bore of 8·12 inches, and in other respects corresponds with the general features of the ordinary 68-pounder. There are 12 grooves, and, as the shot do not enter these grooves, it allows of a much sharper twist than in ordinary rifled guns. The velocity has been found to be 1640 feet a second; the utmost range has not yet been tested. Messrs. Laird and Company, of Birkenhead, are now building an armor-plated vessel called the Agincourt, for the Government, and it was agreed to test the gun against a section of the side of that vessel. It was intended to make the trial last Saturday, but unfavorable weather prevented its being proceeded with. On Monday morning, however, the experiments commenced. The target consisted of an outer plate, 7 feet square and 5½ inches thick, of rolled iron; next came 9 inches of teak, then an inner plate or skin three-fourths of an inch thick, then angle-iron and ribbing, and finally a backing up with timber balks and supports 18 inches thich. The plates were stated, on competent authority, to be the best that can be made of rolled iron. Messrs. Laird supplied the whole, and expressed confidence in the strength of the target. The gun was fired in the presence of Mr. James Mackay, Mr. D. Mackay, Mr. Bouck, and other gentlemen, shortly after 8 o'clock in the morning. There is a little novelty in the mode of loading. Between the powder and the shot a bag of sawdust is placed, Mr. Mackay's object being to give, by means of this substance, a slight movement to the shot before the whole of the powder is ignited. This, without diminishing the force of the charge, prevents a heavy concussion, and renders the danger of bursting considerably less; so much so, that the inventor expresses his confidence that a charge of 100 pounds of powder would not damage the gun. The recoil of the gun, as well as the report is comparatively small. On Monday the gun was charged with 30 pounds of powder and a cast steel shot, weighing 167 pounds, made by Messrs. Thomas Firth and Son, of Sheffield. The range was 200 yards. The shot struck the target with a dull thud a little below the bull's eye on the right, and in the very strongest part, where it was backed up by the rib of the ship's side, the angle-iron, and the timber balk. At the point of impact a perfectly circular hole was cut. The shot then powdered the teak, passed through the inner skin and the angle-iron, shattered the timber balk into fragments, and was picked up 82 yards beyond the target, together with a circular piece of iron armor, about 80 pounds weight, which it had carried with it through the back supports. The sand showed that it had spun to the last. About 70 fragments of iron, bolts, and fragments of the inner skin and angle-iron were picked up 100 yards from the target. The shot when found was reduced from 13 inches to 11 inches in length, and increased about an inch-and-a-half in diameter at the end which struck the target. The other end was uninjured. Tho whole target was forced back about 6 inches, and so much deranged that more shots were not fired.

On Radiation through the Earth's Atmosphere. By JOHN TYNDALL, Esq., F.R.S. Prof. Nat. Phil., Royal Institution.

Nobody ever obtained the idea of a line from Euclid's definition that it is length without breadth. The idea is obtained from a real physical line drawn by a pen or pencil, and therefore possessing width; the idea being afterwards brought, by a process of abstraction, more nearly into accordance with the conditions of the definition. So also with regard to physical phenomena; we must help ourselves to a conception of the invisible by means of proper images derived from the visible, afterwards purifying our conceptions to the needful extent. Definiteness of conceptions, even though at some expense to delicacy, is of the greatest utility in dealing with physical phenomena. Indeed it may be questioned whether a mind trained in physical research, can at all enjoy peace, without having made clear to itself some possible way of conceiving of those operations which lie beyond the boundaries of sense, and in which sensible phenomena originate.

When we speak of radiation through the atmosphere, we ought to be able to affix definite physical ideas, both to the term atmosphere and the term radiation It is well known that our atmosphere is mainly composed of the two elements, oxygen and nitrogen. These elementary atoms may be figured as small spheres scattered thickly in the space which immediately surrounds the earth. They constitute about 99½ per cent. of atmosphere. Mixed with these atoms we have others of a totally different character; we have the molecules, or atomic groups, of carbonic acid, of ammonia, and of aqueous vapor. In these substances diverse atoms have coalesced to form little systems of atoms. The molecule of aqueous vapor, for example, consists of two atoms of hydrogen united to one of oxygen; and they mingle as little triads among the monads of oxygen and nitrogen, which constitute the great mass of the atmosphere.

These atoms and molecules are separate; but in what sense? They are separate from each other in the sense in which the individual fishes of a shoal are separate. The shoal of fish is embraced by a common medium, which connects the different members of the shoal, and renders intercommunication between them possible. A medium also embraces our atoms; within our atmosphere exists a second, and a finer atmosphere, in which the atoms of oxygen and nitrogen hang like suspended grains. This finer atmosphere unites not only atom with atom, but star with star; and the light of all suns, and of all stars, is in reality a kind of music propagated through this interstellar air. This image must be clearly seized, and then we have to advance a step. We must not only figure our atoms suspended in this medium, but we must figure them vibrating in it. In this motion of the atoms consists what we call their heat. "What is heat in us," as Locke has perfectly expressed it, "is in the body heated nothing but motion." Well we must figure this motion communicated to the medium in which the atoms swing, and sent in ripples through it with inconceivable velocity to the bounds of space. Motion in this form, unconnected with ordinary matter, but speeding through the interstellar medium, receives the name of Radiant Heat; and, if competent to excite the nerves of vision, we call it Light.

Aqueous vapor was defined to be an invisible gas. Vapor was permitted to issue horizontally with considerable force from a tube connected with a small boiler. This track of the cloud of condensed steam was vividly illuminated by the electric light. What was seen, however, was not vapor, but vapor condensed to water. Beyond the visible end of the jet the cloud resolved itself into true vapor. A lamp was placed under the jet at various points; the cloud was cut sharply off at that point, and when the flame was placed near the efflux orifice the cloud entirely disappeared. The heat of the lamp completely prevented precipitation. This same vapor was condensed and congealed on the surface of a vessel containing a freezing mixture, from which it was scraped in quantities sufficient to form a small snow-ball. The beam of the electric lamp, moreover, was sent through a large receiver placed on an air-pump. A single stroke of the pump caused the precipitation of the aqueous vapor within, which became beautifully illuminated by the beam; while, upon a screen behind, a richly colored halo due to diffraction by the little cloud within the receiver flashed forth.

The waves of heat speed from our earth through our atmosphere towards space. These waves dash in their passage against the atoms of oxygen and nitrogen, and against the molecules of aqueous vapor. Thinly scattered as these latter are, we might naturally think meanly of them as barriers to the waves of heat. We might imagine that the wide spaces between the vapor molecules would be an open door for the passage of the undulations; and that if those waves were at all intercepted, it would be by the substances which form 99½ per cent. of the whole atmosphere. Three or four years ago, however, it was found by the speaker that this small modicum of aqueous vapor intercepted fifteen times the quantity of heat stopped by the whole of the air in which it was diffused. It was afterwards found that the dry air then experimented with was not perfectly pure, and that the purer the air became the more it approached the character of a vacuum. and the greater, by comparison, became the action of the aqueous vapor. The vapor was found to act with 30, 40, 50, 60, 70 times the energy of the air in which it was diffused; and no doubt was entertained that the aqueous vapor of the air which filled the Royal Institution theatre, during the delivery of the discourse, absorbed 90 or 100 times the quantity of radiant heat which was absorbed by the main body of the air of the room.

Looking at the single atoms, for every 200 of oxygen and nitrogen there is about 1 of aqueous vapor. This 1, then, is 80 times more powerful than the 200; and hence, comparing a single atom of oxygen or nitrogen, with a single atom of aqueous vapor, we may infer that the action of the latter is 16,000 times that of the former. This was a very astonishing result, and it naturally excited opposition, based on the philosophic reluctance to accept a result so grave in consequences before testing it to the uttermost. From such opposition a discovery, if it be worth the name, emerges with its fibre strengthened; as the human character gathers force from the healthy antagonisms of active life. It was urged, that the result was on the face of it improbable; that there were, moreover, many ways of accounting for it without ascribing so enormous a comparative action to aqueous vapor. For example, the cylinder which contained the air in which these experiments were made, was stopped at its ends by plates of rock-salt, on account of their transparency to radiant heat. Rock-salt is hygroscopic; it attracts the moisture of the atmosphere. Thus, a layer of brine readily forms on the surface of a plate of rock-salt; and it is well-known that brine is very impervious to the rays of heat. Illuminating a polished plate of salt by the electric lamp, and casting, by means of a lens, a magnified image of the plate upon a screen, the speaker breathed through a tube, for a moment on the salt; brilliant colors of thin plates (soap-bubble colors) flashed forth immediately upon the screen—these being caused by the film of moisture which overspread the salt. Such a film, it was contended, is formed when undried air is sent into the cylinder; it was, therefore, the absorption of a layer of brine which was measured, instead of the absorption of aqueous vapor.

This objection was met in two ways. First, by showing that the plates of salt when subjected to the strictest examination show no trace of a film of moisture. Secondly, by abolishing the plates of salt altogether, and obtaining the same results, in a cylinder open at both ends.

It was next surmised, that the effect was due to the impurity of the London air, and the suspended carbon particles were pointed to as the cause of the opacity to radiant heat. This objection was met by bringing air from Hyde Park, Hampstead Heath, Primrose Hill, Epsom Downs, a field near Newport in the Isle of Wight, St. Catharine's Down, and the sea-beach near Black Gang Chine. The aqueous vapor of the air from these localities intercepted at least seventy times the amount of radiant heat absorbed by the air in which the vapor was diffused. Experiments made with smoky air proved that the suspended smoke of the atmosphere of West London, even when an east wind pours over it the smoke of the city, exerts only a fraction of the destructive powers exercised by the transparent and impalpable aqueous vapor diffused in the air.

The cylinder which contained the air through which the calorific rays passed was polished within, and the rays which struck the interior surface were reflected from it to the thermo-electric pile which measured the radiation. The following objection was raised:—You permit moist air to enter your cylinder; a portion of this moisture is condensed as a liquid film upon the interior surface of your tube; its reflective power is thereby diminished; less heat therefore reaches the pile, and you incorrectly ascribe to the absorption of aqueous vapor an effect which is really due to diminished reflection of the interior surface of your cylinder.

But why should the aqueous vapor so condense? The tube within is warmer than the air without, and against its inner surface the rays of heat are impinging. There can be no tendency to condensation under such circumstances. Further, let five inches of undried air be sent into the tube—that is, one-sixth of the amount which it can contain. These five inches produce their proportionate absorption. The driest day on the driest portion of the earth's surface would make no approach to the dryness of our cylinder when it contains only five inches of air. Make it 10, 15, 20, 25, 30 inches: you obtain an absorption exactly proportional to the quantity of vapor present. It is next to a physical impossibility that this could be the case if the effect were due to condensation. But lest a doubt should linger in the mind, not only were the plates of rock-salt abolished, but the cylinder itself was dispensed with. Humid air was displaced by dry, and dry air by humid in the free atmosphere; the absorption of the aqueous vapor was here manifest, as in all the other cases.

No doubt, therefore can exist of the extraordinary opacity of this substance to the rays of obscure heat; and particularly such rays as are emitted by the earth after it has been warmed by the sun. It is perfectly certain that more than ten per cent. of the terrestrial radiation from the soil of England is stopped within ten feet of the surface of the soil. This one fact is sufficient to show the immense influence which this newly discovered property of aqueous vapors must exert on the phenomena of meteorology.

This aqueous vapor is a blanket more necessary to the vegetable life of England than clothing is to man. Remove for a single summer night the aqueous vapor from the air which overspreads this country, and you would assuredly destroy every plant capable of being destroyed by a freezing temperature. The warmth of our fields and gardens would pour itself unrequited into space, and the sun would rise upon an island held fast in the iron grip of frost. The aqueous vapor constitutes a local dam, by which the temperature at the earth's surface is deepened: the dam, however, finally overflows, and we give to space all that we receive from the sun.

The sun raises the vapors of the equatorial ocean; they rise, but for a time a vapor screen spreads above and around them. But the higher they rise, the more they come into the presence of pure space; and when, by their levity, they have penetrated the vapor screen, which lies close to the earth's surface, what must occur?

It has been said that, compared atom for atom, the absorption of an atom of aqueous vapor is 16,000 times that of air. Now the power to absorb and the power to radiate are perfectly reciprocal and proportional. The atom of aqueous vapor will therefore radiate with 16,000 times the energy of an atom of air. Imagine then this powerful radiant in the presence of space, and with no screen above it to check its radiation. Into space it pours its heat, chills itself, condenses, and the tropical torrents are the consequence. The expansion of the air, no doubt, also refrigerates it; but in accounting for those deluges, the chilling of the vapor by its own radiation must play a most important part. The rain quits the ocean as vapor; it returns to it as water. How are the vast stores of heat set free by the change from the vaporous to the liquid condition disposed of? Doubtless in great part they are wasted by radiation into space. Similar remarks apply to the cumuli of our latitudes. The warmed air, charged with vapor, rises in columns, so as to penetrate the vapor screen which hugs the earth; in the presence of space, the head of each pillar wastes its heat by radiation, condenses to a cumulus, which constitutes the visible capital of an invisible column of saturated air.

Numberless other meteorological phenomena receive their solution by reference to the radiant and absorbent properties of aqueous vapor. It is the absence of this screen, and the consequent copious waste of heat, that causes mountains to be so much chilled when the sun is withdrawn. Its absence in Central Asia renders the winter there almost unendurable; in Sahara the dryness of the air is sometimes such, that though during the day "the soil is fire and the wind is flame," the chill at night is painful to bear. In Australia, also, the thermometric range is enormous, on account of the absence of this qualifying agent. A clear day, and a dry day, moreover, are very different it is charged with aqueous vapor, and on such occasions great chilling cannot occur by terrestrial radiation. Sir John Leslie and others have been perplexed by the varying indications of their instruments on days equally bright—but all these anomalies are completely accounted for by reference to this newly discovered property of transparent aqueous vapor. Its presence would check the earth's loss; its absence, without sensibly altering the transparency of the air, would open wide a door for the escape of the earth's heat into infinitude.

Proc. Royal Ins., Jan. 23, 1863.

Application by Professor Pepper and another for an injunction to restrain Myers and another from performing the Ghost Illusion.

From the London Mechanics' Magazine, November, 1863.

This was an application for an injunction to restrain the defendant from performing the celebrated "Ghost" illusion. The plaintiffs were Professor Pepper and Joseph Heath, of Liverpool, and the circumstances of the case were as follows (according to the plaintiffs' version): —Mr. Pepper, with one Henry Dircks, were the first inventors of "improvements in apparatus to be used in the exhibition of dramatic and other like performances," and in August, 1863, letters patent were granted, giving them exclusive right of using their invention for 14 years. The method by which the illusion is produced is as follows:—The nature and object of the invention is, by a peculiar arrangement of apparatus, to associate on the same stage a phantom or phantoms with a living actor or actors, so that the two may act in concert. The arrangement of the theatre requires, in addition to the ordinary stage, a second one at a lower level, hidden from the audience as far as direct vision is concerned; the hidden stage is to be strongly illuminated by artificial light, and capable of being darkened instantaneously, whilst the ordinary stage and the theatre remain illuminated by ordinary lighting. A large glass screen is placed on the ordinary stage and in front of the hidden one. The spectators will thus not observe the glass screen, but will see the actors on the ordinary stage through it as if they were not there; nevertheless, the glass will serve to reflect to them an image of the actors on the hidden stage when these are illuminated, but this image is made immediately to disappear by darkening the hidden stage. The glass screen is set in a frame, so that it can be readily moved to the place required, and it is to be set at an inclination to enable the spectators, whether in the pit, boxes, or gallery, to see the reflected image. The glass is adjustable, and it is readily adjusted to the proper inclination by having a person in the pit and another in the gallery to inform the party who is adjusting the glass when they see the image correctly. The hidden stage is capable of being closed at the top by trap-doors. When the trap-doors are closed actors on the ordinary or visible stage can pass freely to and fro above the lower or hidden stage. The ordinary stage and trap-doors are covered with green baize, or other dark material, so that when the trap-doors are opened, the audience, even those in the gallery, will not readily be able to perceive the opening. The actors things. The atmosphere may possess great visual clearness, while or objects corresponding with the phantom images which it is desired to represent to the audience, are strongly illuminated by the lime-light, or the electric-light, or other powerful illuminating means may be employed. This light must accompany the actor in any movement he has to make. The hidden stage and the lanterns may be mounted on a carriage on rails, so that when it is necessary for the phantom actor or object on the lower stage to be moved, the lanterns may be caused to move also, or the lanterns may remain stationary whilst the actor moves, provided the whole space through which he moves is sufficiently illuminated. The lanterns are to be provided with means for instantaneously extinguishing or making the light, and for reproducing it, so that the phantom may be made to disappear and re-appear at pleasure, whilst the audience and the stage will be more or less lighted in the ordinary manner, according to the effects desired to be obtained. For this purpose, a board is employed which is capable of being raised into a position so as to entirely cut off the light from the hidden stage when desired, or an ordinary opaque shade attached to the lantern may be used for the purpose, or when using the lime-light the desired effects are caused by gradually or instantaneously (as the case may require) cutting off the supply of gases, and the phantom image may, by any of these means, be caused gradually or instantaneously to fade away. When the trap-doors over the hidden stage are open, the part thereof assists in hiding the lanterns and the opening from the audience. The part acts to screen the lanterns from the audience, and also to ensure that any actor or object on the hidden stage shall not accidentally appear above the level of the visible stage. The phantom actor when standing on the stage leans against the screen, which is inclined so as to be parallel with the glass screen, and is covered with black velvet or other dark material, as is also the stage, in order that no image of either the screen or the stage may be seen in the reflection. The glass screen is a large inclined sheet of plate-glass on the ordinary stage, of sufficient size to reflect the full length of the actors or objects on the hidden stage to the audience in the pit, boxes, and galleries of the theatre. The hidden stage is between the glass and the audience. The glass is mounted in a swing frame so that it may be adjusted to the angle required by screws, or ropes and pulleys, or otherwise. The glass is to be set at such an inclination as to bring the reflected image to the level of the visible stage. This will enable the spectators, whether in the pit, boxes, or gallery, to see the reflected image without any obstruction to the view above the foot-lights, and it will be visible from all parts of the house except those extreme positions which cannot command a view through the glass of that part of the stage where the image is reflected. The glass being in a swing-frame the proper angle of inclination is ascertained experimentally by having persons in different parts of the house to say when the image is shown to them correctly. The scenery is so disposed as to conceal the frame of the glass, and we prefer that the glass should be able to descend into an opening or box beneath the stage, in which case we counterbalance the glass and frame so that they may easily be raised into the position desired by means of a rope, by which, aided by the bolts, the glass is supported in the required position. In this case the glass may either be adjusted when screened from the audience, and remain in position during the scenes, or (the proper angle of inclination having been previously ascertained by experiment) the glass may be raised on to the ordinary or visible stage and placed in position whilst the scene is before the eyes of the audience, under a subdued light, without the movement being observed, for which latter purpose the top bar of the frame of the glass should be made very light, or be omitted altogether. This arrangement admits of an actor on the visible stage passing across the space which the glass is to occupy, and this he can do just before the appearance of the phantom; and then immediately the glass is run up, the trap-doors are opened, the actor or image on the hidden stage is illuminated, and the phantom appears. This arrangement will render it less likely that the audience should imagine that there is anything interposed between them and the actors, than if the glass plate remained permanently in position during the scene. The hidden or lower stage may be provided with a trunk or well-hole, up which an actor can rise; he will then appear as a spectre rising out of the visible stage. The lanterns may be provided with colored glasses in order to heighten the effect. As the actors on the visible stage do not themselves see the spectral images, marks should be placed on the stage, or other indications made, in order that they may know the position which the spectres appearing to the audience are to occupy. In order to appear upright upon the visible stage, the actor or object on the hidden stage should be inclined so as to be as nearly as practicable parallel with the surface of the glass. In affecting this, assistance is afforded by the screen of the hidden stage. Several sheets of glass may be employed at the same time, if one is not of sufficient width to cover the different parts of the stage at which it is desired that the spectre should appear, the interval or junction being concealed by the introduction of a tree or column, or some other pieces of scenery.—The injunction was granted.

A New Alloy applicable to the manufacture of all metal articles.

From the London Mining Journal, No. 1499.

A new alloy described as applicable to the manufacture of all metal articles, bells, hammers, anvils, rails, and non-cutting tools, has been patented by Mr. M. H. Micolon, of Paris. The alloy consists of iron ings or tin waste, 80 parts of steel, 4 parts of manganese, and 4 parts with manganese or borax. The patentee takes 20 parts of iron turn- of borax; but these proportions may be varied. When it is desired to increase the tenacity of the alloy, 2 or 3 parts of wolfram are added. When the cupola is ready, the iron and steel are poured in, and then the manganese and borax; finally the vessel is filled up with coke; the metal is thus in direct contact with the fuel in the cupola, and by quickly running the fused mass into moulds, bells which possess the sonority of silver, whilst the cost is less than bronze, may be obtained.

The Great Eastern as a Screw Steamer only.

From the London Mining Journal, No. 1494.

The Quarterly Journal of Science contains an article on "Steam Navigation; its Rise, Progress, and Prospects," by Mr. Martin Samuelson, C. E., in which the author suggests that the paddle engines of the *Great Eastern* should be removed; and in order to show the great advantages she would then possess over the other large steamers, he gives the following comparative statement of the capacities of the *Persia* and the *Great Eastern*, under the conditions suggested by him: —"With her paddle engines removed, the *Great Eastern* would carry about 7400 tons of measurement goods, and 12,000 tons of coal (more cargo and less coal in proportion). She would burn about 200 tons of coal per diem, and steam 9 knots per hour. The *Persia* carries 1257 tons of measurement goods, and 1700 tons of coal, and, burning about 150 tons per day, attains an average speed of 12 knots per hour. Thus, if we were to take into consideration the increased speed attained by the *Persia* over the *Great Eastern*, we should have to take the quasi-consumption of the latter, not at 200, but at 260 tons per day. Now, let us compare the work as it would be performed by the two boats, with the coal required by each, and we shall find that the *Persia*, carrying 1257 tons of goods, and consuming 150 tons of coal per day, burns 270 ℔s. of coal per day for every ton of goods carried by her. Whilst the *Great Eastern*, carrying 7400 tons of goods, and consuming 268 tons of coal per day, would only burn 81 ℔s. of coal per day, for every ton of goods carried. This comparative statement exhibits in a general manner how great is the advantage of a screw over a paddle steamer for trading purposes, but as far as the *Great Eastern* is concerned, we do not hesitate to say that with appropriate internal arrangements she could be made to carry at least 10,000 tons of measurement goods; that with the screw alone and a suitable rig, she would, in an average state of the weather, attain a speed of 10 knots an hour; whilst with a good wind she would keep pace with, if not outstrip, the fastest paddle steamer afloat. A comparison of the trans-Atlantic mail paddle boats, supported by a subsidy, with the screw boats in the same service not so endowed, would further confirm the statement of the superior economy of the screw.

For the Journal of the Franklin Institute.

On Joule's Equivalent of Heat. By John W. Nystrom, C. E.

In reading an article on "Economy in the Use of Steam," page 313, of this Journal, I came across a passage where the equivalent of power is divided by Joule's equivalent of heat, and the result called "units of heat transmitted into power." This drew my attention to another error in the article on Steam Boiler Explosion, page 328, where I divide units of heat by Joule's equivalent, and call the result horse-power. Both of these results are wrong, and ought to be corrected.

Joule's equivalent of heat is work, or *vis viva*, and not power; it is foot pounds of work, and not foot pounds of power.

Joule's equivalent divided by time will be power. If one equivalent of heat is generated per second, the horse-power will be

$$\text{H} = \frac{772}{550} = 1{\cdot}4363 \text{ horses,}$$

when 550 lbs. raised one foot per second is one horse-power.

Let us assume the unit for work to be that which one horse-power can accomplish in the time of one hour, which will be about the same as that of a man working one day, and let W represent the number of such units; then the first formula for horse-power, page 328, will be

$$\text{W} = \frac{31 \times 180^\circ \times 5{\cdot}5 \times 62{\cdot}5 \times 772}{550 \times 3600} = 747{\cdot}875 \text{ units of work,}$$

or it would require 748 men to work one day to do the same amount of damage as that of the explosion.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, May 19, 1864.

William Sellers, President, in the chair.

$$\left.\begin{array}{l}\left.\begin{array}{l}\text{John H. Towne,}\\ \text{Fairman Rogers,}\end{array}\right\}\text{Vice Presidents,}\\ \text{John F. Frazer, Treasurer,}\end{array}\right\}\text{present.}$$

Washington Jones, Recording Secretary.

The minutes of the last meeting were read and approved.

Donations to the Library were received from the Royal Astronomical Society, the Royal Geographical Society, the Statistical Society, and the Society of Arts, London; la Société Industrielle de Mullhouse, France; de Neider-osterreichischen-Gewerbe Vereines, Vienna, Austria; Dr. E. M. Dingler, Augsburg, Germany; Hon. Wm. D. Kelley, and Hon. Geo. Middleton, U. S. Congress, and the Smithsonian Institution, Washington, D. C.; the Young Men's Association, Buffalo, New York; Hon. George Connell, Pennsylvania Senate, Harrisburg, Penna.; the American Philosophical Society, Isaac B. Garrigues, Esq., Prof. John F. Frazer, and Messrs. William Sellers & Co., Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's report for April was read.

The Board of Managers and Standing Committees reported their minutes.

The Special Committee to revise the By-Laws, reported progress, and at their request the following gentlemen were added to the Committee: Messrs. Frederick Fraley, George Erety, George Harding, John C. Cresson, Samuel V. Merrick, Henry G. Morris, and William Sellers.

The Special Committee to investigate the question of a proper system of screw threads, bolt heads, and nuts, reported progress, and at their request, Messrs. Robert Briggs and Algernon Roberts were added to the Committee.

The Special Committee on the new Charter, reported the Charter as passed by the Legislature, which was read, and, on motion, accepted.

The Special Committee on steam expansion, reported progress.

On motion, it was

Resolved, That when the Institute adjourn, they adjourn to meet on Thursday evening, June 9, proximo, to receive the report of the Committee on the revision of the By-Laws, and consider the same.

One resignation of membership in the Institute, was read and accepted.

Candidates for membership in the Institute (13) were proposed, and the candidates proposed at the last meeting (7) were duly elected.

Mr. J. B. Britton exhibited a model of his patented Furnace for Roasting Ores, etc.

In this furnace the ore is contained in a central vertical chamber, across which the products of combustion from the fire-place are caused to pass regularly in one direction. By a discharging device the rate of descent of the ore through the chamber can be regulated at pleasure, so that each particle of ore can be submitted to the action of the fire for any length of time desired. By carrying the draft *across* the chamber the products of combustion are brought in direct contact with every portion of the ore, and the sulphurous and other deleterious vapors are taken direct to the chimney, and are not carried among the particles of unroasted ore, as in ordinary kilns.

A model of Harthol & Lee's "American Farm Gate" was exhibited. In this gate the bars are jointed together at the point of their intersection, and all the horizontal bars are jointed at one end to the side post, so that when these bars are raised to a vertical position the others will be brought close together, leaving the passage way between the posts clear.

Barry, Waples & Singleton's Bilge Water Gauge, for measuring the depth of water in the holds of vessels, etc., was exhibited.

This gauge consists of an inverted siphon containing mercury, the short leg of the siphon being about four times the diameter of the other, and having connected to it one end of a copper pipe, the other end of which communicates with an air chamber in the hold. The air chamber is a pipe about 14 inches long and one inch inside diameter, open at the bottom and closed at the top. When the water rises in the hold, the air in the air chamber is compressed, and this pressure is communicated to the mercury in the siphon, which, by its height, indicates the depth of water in the hold.

This gauge is also admirably adapted for showing the depth of water in a screw or sectional dock, reservoir, or cistern, there being no practical limit to its capacity, as it will show as accurately the depth of fifty feet as one foot.

Mr. James D. Rice submitted for examination several forms of Lightning Rod Points, and mode the following remarks:

A new invention is not claimed for any of the lightning rod points exhibited, but simply a modification of a point constructed by Professor Robert T. Hare.

Through the kindness of Judge Hare, I have obtained this point for exhibition this evening: at a future time it is to be presented to the Franklin Institute to be deposited in their model-room. This point is constructed of 18 small copper wires, soldered to the upper end of an iron rod, the junction surrounded by a zinc ball; the object of which is to preserve the copper points from oxidation. This point was made over 30 years ago—as I find it described in Professor Hare's Chemistry—and, as may be seen by examination, the copper wires are in good order.

I exhibit two points of my *own* construction, differing somewhat from Professor Hare's but upon the same principle. One of these was placed upon a flag-pole upon the American Mechanics' Hall, and was struck by lightning last summer; one of the copper points shows slight marks of fusion, but not injured for further use; no other effects could be traced upon the rod or building. Here is another point which has been struck, a platina point, of M'Allister's manufacture; the platina is all melted off, and the rest of the points much torn by the explosion. This I have found to be the case in all instances with the platina point—doubtless owing to its very *low conducting power* and the *form* in which it is used. Another objection to this point is the very imperfect method of connecting the point to the rod. I consider the platina points of little value for lightning rods. Points should be so constructed as to be uninjured when repeatedly struck.

In replying to the remarks of Professor Frazer, my opinion is, that although lightning conductors in complete order *do* silently convey electricity from a cloud, yet they cannot in this way prevent a stroke at all times; if they could, lightning should never strike in this city where there are many hundred rods, many of which are as complete as possible, and yet it does strike from 3 to 6 times or more every season. Again, whoever has watched the lightnings flash during storms, has observed it to pass horizontally a long distance—probably many miles—and then descend to the earth; in such cases the clouds being too far off to be discharged silently by the conductor; it then becomes the office of the lightning rod to receive and convey the stroke harmlessly into the earth. In an *instant* it may pass from the clouds, say 10 or 15 miles distant, to one directly over us, and thence to the earth, destroying life and property without descrimination, unless protected by suitable conductors.

A word or two relative to insulation of rods; no good authority sanctions their use. Professor Hare's rod erected by himself, on the contrary, is soldered to the metal roof, making the roof a part of the conductor. This principle I have adopted on many large buildings in this city, such as railroad depots, market-houses, Girard College buildings, &c.

A Comparison of some of the Meteorological Phenomena of MAY, 1864, *with those of* MAY, 1863, *and of the same month for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	May, 1864.	May, 1863.	May, for 13 years.
Thermometer—Highest—degree, .	86·00°	90·00°	90·00°
“ “ date, .	10th & 31st.	23d.	7th’60;23d’63
“ Warmest day—Mean,	78·17	27·84	79·83
“ “ “ date,	9th.	23d.	23d, 1863.
“ Lowest—degree, .	41·00	40·00	35·00
“ “ date, .	3d.	7th.	7th, 1854.
“ Coldest day—Mean,	49·67	42·50	40·00
“ “ “ date, .	3d.	6th.	3d, 1861.
“ Mean daily oscillation,	16·61	19·15	17·24
“ “ “ range, .	5·03	5·43	5·50
“ Means at 7 A.M., .	62·53	59·32	58·57
“ “ 2 P.M., .	73·35	71·76	69·93
“ “ 9 P.M., .	65·15	62·64	61·58
“ “ for the month,	67·01	64·57	63·36
Barometer—Highest—Inches, .	30,000 in.	29·975 in.	30·338 in.
“ “ date, . .	5th.	21st.	4th, 1852.
“ Greatest mean daily press.	29·978	29·962	30·273
“ “ date, . .	5th.	21st.	5th, 1852.
“ Lowest—Inches, .	29·379	29·294	29·096
“ “ date, . .	10th.	31st.	27th, 1861.
“ Least mean daily press.,	29·437	29·343	29·243
“ “ date, . .	10th.	31st.	27th, 1861.
“ Mean daily range, .	0·108	0·085	0·120
“ Means at 7 A.M., .	29·663	29·783	29·812
“ “ 2 P.M., .	29·627	29·736	29·775
“ “ 9 P.M., .	29·665	29·769	29·779
“ “ for the month, .	29·652	29·763	29·795
Force of Vapor—Greatest—Inches,	0·714 in.	0·070 in.	0·771 in.
“ “ “ date, .	24th.	30th.	14th, 1854.
“ “ Least—Inches, .	·202	·189	·069
“ “ “ date, .	3d.	5th.	2d, 1861.
“ “ Means at 7 A.M., .	·423	·375	·357
“ “ “ 2 P.M., .	·452	·365	·375
“ “ “ 9 P.M., .	·461	·406	·380
“ “ “ for the month,	·445	·382	·371
Relative Humidity—Greatest—per ct.	97 per ct.	91 per ct.	100·0 per ct.
“ “ “ date,	26th.	7th.	Often.
“ “ Least—per ct.,	32·0	28·0	16·0
“ “ “ date, .	28th.	22d.	5th, 1855.
“ “ Means at 7 A.M.,	73·2	72·3	71·4
“ “ “ 2 P.M.,	54·5	48·3	51·2
“ “ “ 9 P.M.,	73·6	69·8	68·5
“ “ “for the month	67·1	63·5	63·7
Clouds—Number of clear days,* .	7	15	10·4
“ “ cloudy days, .	24	16	20·6
“ Means of sky cov’d at 7 A.M.,	62·9 per ct.	51·0 per ct.	58·3 per ct.
“ “ “ “ 2 P.M.,	69·0	55·5	60·3
“ “ “ “ 9 P.M.,	58·4	38·1	46·5
“ “ “ for the month,	63·4	48·2	55·0
Rain Amount, .	9·042 in.	4·792 in.	4·609 in.
No. of days on which Rain or Snow fell,	12	11	12·7
Prevailing Winds—Times in 1000	S 22°23′W·105	N 77°8′W·877	N74°38′W·105

* Less than one-third covered at the hours of observation.

A Comparison of some of the Meteorological Phenomena of the SPRING *of* 1864, *with that of* 1863, *and of the same Season for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By J. A. KIRKPATRICK, A. M.

	Spring, 1864.	Spring, 1863.	Spring, for 13 years.
Thermometer—Highest—degree, .	86·00°	90·00°	90·00°
“ “ date, .	May 10 & 31,	May 23d.	My 7'60:My23 '63
“ Warmest day—Mean,	78·19	79·83	78·83
“ “ “ date,	May 9,	May 23d.	May 23, 1860.
“ Lowest—degree, .	20·00	15·00	4·00
“ “ date, . .	March 22d.	March 5th.	March 10 '56.
“ Coldest day—Mean,	27·33	24·17	11·50
“ “ “ date, .	March 22d.	March 5th.	March 10 '56.
“ Mean daily oscillation,	15·19	15·95	16·20
“ “ “ range, .	5·14	5·77	5·98
“ Means at 7 A. M., .	47·26	45·19	46·47
“ “ 2 P. M., .	58·00	55·68	57·86
“ “ 9 P. M., .	50·88	48·62	50·30
“ “ for the Spring,	52·05	49·83	51·54
Barometer—Highest—Inches, .	30·131 in.	30·384 in.	30·522 in.
“ “ date, . .	March 3d.	March 21st.	March 3d, '52.
“ Greatest mean daily press.	30·090	30·311	30·458
“ “ date, . .	March 3d.	March 20th.	April 3d, '54.
“ Lowest—Inches, . .	29·184	29·260	28·884
“ “ date, . .	March 30.	April 2d.	Ap. 21st. '52.
“ Least mean daily press.,	29·246	29·343	28·959
“ “ date, . .	March 30.	May 31st.	Ap. 21st, '52.
“ Mean daily range, .	0·135	0·154	0·161
“ Means at 7 A. M., .	29·705	29·818	29·822
“ “ 2 P. M., .	29·661	29·772	29·777
“ “ 9 P. M., .	29·706	29·819	29·808
“ “ for the Spring,	29·691	29·803	49·802
Force of Vapor—Greatest—Inches,	0·714 in.	0·700 in.	0·771 in.
“ “ “ date, .	May 24th.	May 30th.	May 14, 1854.
“ “ Least—Inches, .	0·664	·050	·023
“ “ “ date, .	March 20,	March 15th.	Mar. 5, 1858.
“ “ Means at 7 A. M.,	·263	·243	·249
“ “ “ 2 P. M.,	·278	·243	·265
“ “ “ 9 P. M.,	·286	·264	·269
“ “ “ for the Spring,	·276	·250	·261
Relative Humidity—Greatest—per ct.,	97·0 per ct.	96·0 per ct.	100·0 per ct.
“ “ “ date,	May 26th.	April 24th.	Often.
“ “ Least—per ct.,	18·0	15.0	13·0
“ “ “ date, .	Mar. 9; Ap. 7.	April 24th.	April 13, '52.
“ “ Means at 7 A.M.,	71·9	72·3	72·2
“ “ “ 2 P.M.,	52·4	51·7	51·8
“ “ “ 9 P.M.,	69·1	69·4	67·9
“ “ “ for Spring,	64·5	64·5	64·0
Clouds—Number of clear days,* .	21	27	28·8
“ “ cloudy days, .	71	65	63·2
“ Means of sky cov'd at 7 A.M.	52·2 per ct.	58·7 per ct.	60·3 per ct.
“ “ “ “ 2 P.M.	68·6	67·7	62·6
“ “ “ “ 9 P.M.	59·2	50·4	48·6
“ “ “ for Spring,	63·3	58·9	57·2
Rain and Melted Snow—Amount,	18·763 in.	18·465 in.	12·816 in.
No. of days on which Rain or Snow fell,	36·	43·	36·5
Prevailing Winds—Times in 1000,	N52°39′W·096	N40°15′W·170	N67°58′W·186

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* Less than one-third covered at the hours of observation.

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